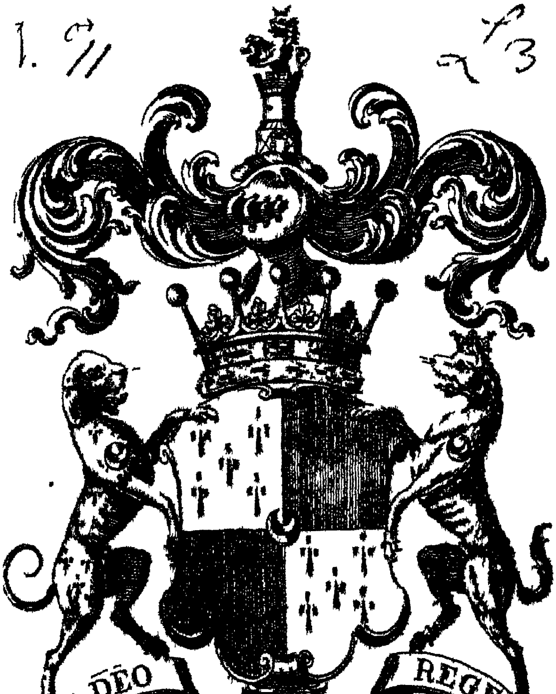

Philip Earl Stanhope.

PHILOSOPHICAL TRANSACTIONS,

OF

THE ROYAL SOCIETY

OF

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VOL. LXVI. For the Year 1776.

PART I.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

CONTENTS

TO

VOL. LXVI. PART I.

IV. *A*

XII. *An*

PHILOSOPHICAL TRANSACTIONS.

I. *On the Nature of the* Gorgonia; *that it is a real Marine Animal, and not of a mixed Nature, between Animal and Vegetable. By* John Ellis, *Esq. F. R. S. in a Letter to* Daniel Solander, *M. D. F. R. S.*

MY DEAR FRIEND,

R. June 29, 1775.

IT was your particular requeſt, before you went to the South Seas, that I ſhould continue my reſearches into the formation and growth of *Zoophytes*, more particularly of thoſe formerly called *Ceratophytons*, now *Gorgoniæ*; and known in Engliſh by the name of ſea-fans, ſea-feathers, and ſea-whips, to which claſs the red coral ſhould be added. This you thought the more neceſſary, as the accounts already publiſhed of them by the illuſtrious Dr. LINNÆUS and Dr. PALLAS ſeemed to make them of a mixed nature in their growth, between animals and vegetables: a thing not eaſily to be reconciled to the uſual operations of nature.

I was ſo fortunate about that time to receive from my right honourable friend the earl of HILLSBOROUGH, a moſt excellent collection of different ſpecies of theſe animals preſerved at the ſea-ſide in ſpirits, by JOHN GREG, eſq. F. R. S. of Dominica. This hath enabled me to ſhew more clearly, that they are true animals, growing up in a branched form, and in no part vegetable.

From the following obſervations it will appear, that the *gorgonia* is an animal of the *polype* kind, reſembling the common freſh water *polype* in many of its qualities, but differing from it in the remarkable circumſtance, of producing from its own ſubſtance a hard and ſolid ſupport, ſerving many of the purpoſes of the bone in other animals. Every one knows, that the common *polype* ſends out its young from its ſide, like buds, which being grown to the form of the parent animal, to which they ſtill adhere, ſend out again their own young, like buds, adhering to themſelves; and this is repeated, till at length the whole acquires a branched appearance, reſembling a vegetable, ſee fig. 1.

The *gorgonia* grows nearly in the ſame manner; and hence ariſes its reſemblance to a ſhrub, which hath given occaſion to the miſtake of placing it in the vegetable kingdom. But though the nature of theſe animals is ſo much like the *polypes*, they differ in ſeveral circumſtances; the moſt remarkable is that which I have already mentioned, the hard bone by which the *gorgonia* is ſupported. This is not formed by any kind of vegetation, but by a concreting juice thrown out from a peculiar ſet of longi-

tudinal

tudinal parallel tubes, running along the internal ſurface of the fleſhy part. In the coats of theſe tubes are a number of ſmall orifices, through which the *oſſeous* liquor (if I may uſe the expreſſion) exudes; and concreting, forms the layers of that hard part of the annular circles, which ſome, judging from the conſiſtence rather than the texture, have erroneouſly denominated wood.

Dr. PALLAS, in his *Elench. Zoophytorum*, p. 162. is of opinion, that the layers of which the wood, as he calls it, of the tougher *gorgoniæ* is compoſed, may be ſeparated into numerous longitudinal fibres; that the longitudinal *ſtriæ*, which frequently appear on its external ſurface, are owing to this ſtructure; and that theſe fibres are in fact hollow, like the wood of trees, the cavity of the tubes being cloſed up, as they become hard and rigid.

I was nearly of the ſame opinion when I was writing my *Eſſay on Corallines*, as may be ſeen in the Philoſophical Tranſactions. vol. XLVIII. p. 18. and alſo p. 504. *t.* 17. where I have compared the herring-bone coralline, which is compoſed of many little tubes, to the growth of ſea-fans and ſea-feathers, now called *gorgoniæ*; and likewiſe in my *Obſervations on the Growth of the red and white Coral*, ſee Philoſophical Tranſactions, vol. XLVIII. p. 504. *t.* 17.; but experience has ſince fully convinced me of the contrary: for upon the ſtricteſt examination with the microſcope, of the internal horny parts of ſeveral of thoſe *gorgoniæ* freſh from the ſea, and immediately preſerved in ſpirits, not the leaſt appearance of tubes within the horny part can be diſcerned, either in the longitudinal or tranſverſe ſections. There

is indeed a regular cannulated appearance on the ſurface; but this ſeems to be only an external moulding, and not formed by a ſeries of longitudinal tubes with interſtices, as in plants; nor is it difficult to explain from whence ſuch a moulding may ariſe. I have obſerved, that the inner ſurface of the fleſhy part contiguous to the bony or horny part, is furniſhed with longitudinal parallel tubes, which through certain pores ſupply the *oſſeous* matter; this being ſoft at firſt, and only afterwards becoming hard, ſo as neceſſarily to take the form of the concave ſurface by which it is cloſely preſſed, and therefore aſſumes a ſtriated appearance. This is plainly ſeen in fig. 2. A. where the ends of the tubes and the ſtriated appearance on the *gorgonia flabellum* are expreſſed; and at fig. 2. B. two of them are magnified.

In the *Iſis hippuris*, or black and white jointed coral, which is very nearly a-kin to this *genus*, theſe tubes are ſtill more clearly to be ſeen, as they are larger and the channels much deeper, ſee fig. 3. where A is a part of the coral of its natural ſize; B is an extremity of one of the branches magnified, with the bony part laid bare; C a part of the ſame, with the bony part taken out, to ſhew the tubes with their internal orifices, through which the *oſſeous* juice is ſuppoſed to exude, and form the layers of the bony and horny part. This formation of the hard part or bone of the ſtem ſeems to be a principal uſe of the longitudinal tubes; but they have another alſo, of great conſequence in the growth of the *gorgonia:* for it is by means of theſe, that the animal ſpreads itſelf downwards over the ſubſtances which ſerve for its baſis, thence

deriving

deriving a firmneſs proportioned to its bulk. By means of theſe likewiſe, it repairs any deficiencies ariſing either from accident or natural decay, by which the life of the whole would be endangered. At fig. 2. C, D, the broken ſtem in the *gorgonia flabellum* is ſtrengthened and made firm by the lateral reticulations being covered over with the horny ſubſtance by means of theſe fleſhy tubes and *polype* ſuckers. This is very different from any natural repairs of broken or wounded branches in trees. Beſides, theſe tubes extend themſelves any way, creeping over every ſubſtance which may ſerve for their ſupport and preſervation of the animal, throwing out the horny or *oſſeous* juice to make the whole texture firmer. This wonderful contrivance of nature is certainly inſtinct in this low order of animals. To give a better idea of this kind of inſtinct, and to ſhew in what it differs from what is called radication in plants, with which ſome people, for want of better information, are apt to confound it, I have given a figure of the manner in which the *fluſtra foliacea* faſtens itſelf to ſhells, ſee fig. 4. This figure is a little magnified, to ſhew the form of the cells, as they have ſpread themſelves over the ſurface of the ſcollop ſhell. The advocates for the vegetation of *zoophytes*, I hope, will be convinced, that the part that ſticks to the ſhell is not a root, but only a ſingle courſe or layer of cells of the ſame animal. As it riſes into leaf-like branches they become double, or two layers of cells, placed in ſuch an oppoſition to one another as to ſtrengthen the whole, like the cells in the honey-comb; and what is very ſingular, the narrow part of the ſtem near the ſhell, often

conſiſts of four or more layers of cells, which the animal, by this kind of inſtinct, moſt certainly applies to ſtrengthen that ſlender part againſt the force of the waves. For another inſtance of the baſe of a *zoophyte* ſpreading downwards to ſecure itſelf, we have an example in the *madrepora muricata*, which is extending itſelf over a dead animal of the ſame ſpecies, ſee fig. 5.

The following remark of Dr. PALLAS will ſhew, that as he conceives the wood or horny ſtem to be compoſed of tubes, ſo he thinks that there is a communication of juices from the *polypiferous* pores on the cortical part to the inſide or horny part, as in trees: for he obſerves, that as the trunk of the *gorgonia* is always proportioned to the ſize of its branches, the wood or horny part of the trunk, notwithſtanding its hardneſs, muſt neceſſarily thrive, grow and increaſe every way, even though the organs of the bark, or ſurrounding fleſhy ſubſtance, at the trunk and baſe are obliterated(a); and hence he concludes, that the trunk muſt receive nouriſhment from the branches, and apprehends, this nouriſhment to be abſorbed and prepared by *polypiferous* pores. Now it is evident, that the idea of the trunk and baſe of a tree growing in thickneſs, when it is diveſted of its ſurrounding bark, is contrary to the known laws of vegetation. The only method of increaſe in the trunks of trees is by

(a) Semper baſeos amplitudo et imi trunci craſſities proportionata magnitudini fruticis reperitur; argumento corneam eorum partem, quam exemplo arborum fruticumque, lignum dicere queat, obſtante duritie ubique vigere, vivere et in omnem dimenſionem creſcere, obſoletis quamvis corticis in trunco et baſi organis. PALLAS, Elench. Zoophyt. p. 161.

the

the appoſition of new layers from the bark, which cannot be produced but while the bark is ſubſiſting.

Nor can the *gorgonia* increaſe in ſize, in thoſe parts where it is deprived either of the fleſh with the *polype* ſuckers, or the ſurrounding fleſhy tubes, which communicate with theſe ſuckers; for theſe ſuckers and tubes are the organs that prepare and depoſite the ſeveral thin layers, which form the ſupport or bony part (here called wood), as I have ſhewn before. If upon examining the internal ſtructure of theſe *zoophytes* it were found, that their growth and fabric any ways reſembled that of vegetables, this would indeed afford a preſumptive argument, that they did participate of a vegetable nature. Yet even in that caſe, it would be much more reaſonable to ſuppoſe them animals of the loweſt order, raiſed but one degree above the vegetable tribe, than to conjecture a monſtrous metamorphoſis repugnant to the general analogy of nature. But the truth is, that although the hard parts of many *gorgoniæ* have very much the external appearance of wood, yet the internal ſtructure differs in the moſt eſſential points from vegetables.

In order to prove this, I have compared different ſections of the *gorgonia* with correſpondent ſections both of ſea and land plants, and find they differ in the following particulars: The longitudinal ſections of the ſtems of the larger *fuci*, ſuch as the *fucus digitatus, eſculentus, nodoſus*, and *ſacharinus*, appear compoſed of parallel jointed tube-like figures, the joints of which are compoſed of gland-like cells; theſe tubular appearances, when highly magnified, are diſcovered to be connected together

together by tranſparent reticulated fibres, or very minute tranſverſe tubes, interwoven with the upright ones. In a horizontal ſection, the ranges of cells, which look like rays from the center, as they approach the bark, grow ſmaller and ſmaller, and moſt probably correſpond with the minute pores which cover the outward ſurface of the plant; for when the ſides of the dry ſtems are ſoaked in water, they quickly imbibe it, and ſoon become full of a gelatinous liquor; all which is totally different from the texture of the *gorgonia.*

We come now to compare them with land plants, ſuch as ſhrubs, to which they are generally ſuppoſed to grow like. The *gorgonia* has no regular ſeries of hollow fibres or little tubes, in what is called the wood, either longitudinal or horizontal. It appears compoſed of a ſort of irregular *laminæ* like horn; the fibres of which take no certain direction, nor preſerve in any two places the ſame thickneſs. It has no ſeries of utricular veſſels, as the tranſverſe veſſels of wood are called by MALPIGHI; or inſertions as they are called by Dr. GREW. Theſe are eſſentially neceſſary, as forming a communication from the bark and the internal parts of the wood quite through. On the contrary, the concentric circles of the *gorgonia* have no connexion with each other; they run like ſo many parallel curves, and are connected by no inſertions or utricular veſſels; but to all have been appearance formed by ſeparate depoſitions of concreting matter. So the ſhells of ſnails and oyſters are formed; their reſpective animals throw out periodically the *oſſeous* juice or *teſtaceous* matter, which adheres to the former ſhell and concretes, and thus

Fig. 1.

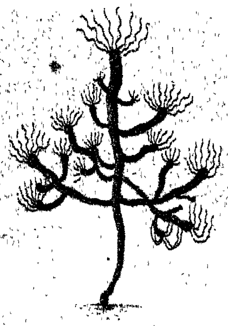
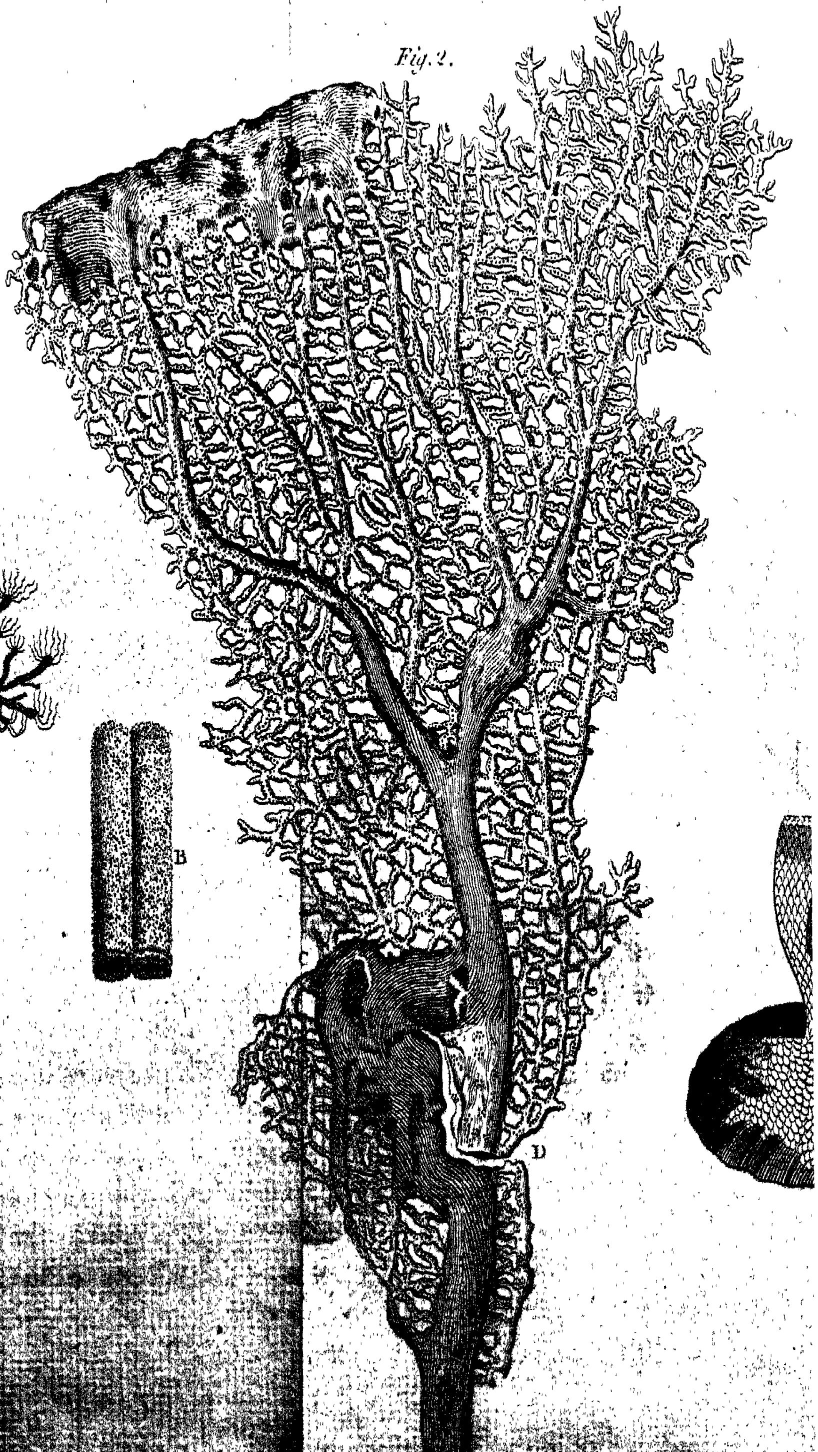


Fig. 2.



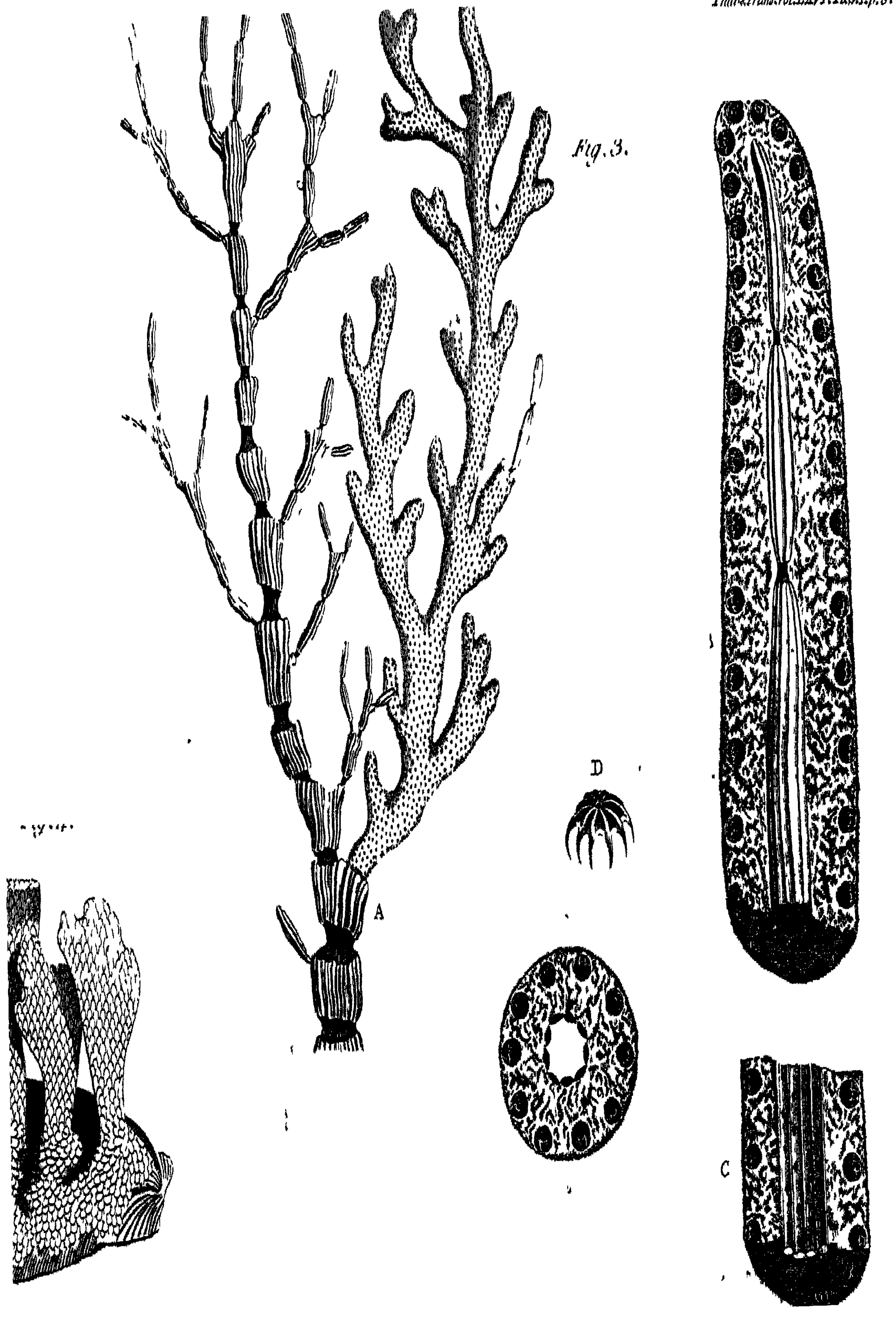


Fig. 3.
A
D
C

thus ſucceſſive layers are produced. In the ſame manner I ſuppoſe the concentric circles of the *gorgonia* to be formed, ſucceſſive layers of juice exuding from the fleſhy tubes that ſurround the hard part or bone of the animal. Thus the ſtem of the *gorgonia verticillata*, or *Minorca white ſea-feather*, is compoſed of different layers of a ſhell-like ſubſtance (ſee fig. 6.), where a broken part of the ſtem is repreſented, and a piece of it magnified, to ſhew that there is evidently no more communication between the different *laminæ* than there is between thoſe of an oyſter-ſhell. In a tranſverſe ſection of the *gorgonia pretioſa*, or true red coral, DONATI has obſerved, Philoſophical Tranſactions, vol. XLVII. p. 97. *t.* 3. *f. d.* "Different lines or annual bands, whereof one part is of "a roſe colour, another yellowiſh, others white, others "more or leſs charged with colours, that form concen-"tric circles like the coats of an onion." This diverſity of colours could hardly have taken place, had there been a circulation of juices through the ſtem; and it was probably owing to the different food which the animals had lived upon at different periods.

There is another *genus* of *zoophyte*, which though it ſwims freely about in the ſea, yet approaches near to the *gorgonia*, and will ſerve further to explain the growth of its ſtem, and that is the *pennatula*, or ſea-pen. This *genus* hath a bone along the middle of the inſide, which is its chief ſupport. This bone receives the ſupply of its *oſſeous* matter by the ſame *polype* mouths, that furniſh it

 with

with nouriſhment. Dr. BOHADSCH has very judiciouſly brought to this *genus* the great Greenland cluſtered *polype* formerly deſcribed by me under that name, and now called *pennatula arctica*. In a croſs ſection of the bone, ſee Philoſophical Tranſactions, vol. XLVIII. tab. XII. F. H. the ſeveral *laminæ* are magnified, to ſhew that they are formed in layers like ſhells, and are not full of tubes as in a vegetable growth These animals are ranged among the vegetating kind, and ſo called by Dr. PALLAS. There is a great affinity between the *gorgonia* and *iſis*, ſo that the increaſe of the bone of the latter will greatly illuſtrate that of the former. The longitudinal ſection of the bone to the ſtem of the *iſis hippuris* will ſhew, that it hath been increaſed in diameter by ſucceſſive layers of ſtony matter that ſurround it, ſee fig. 7. In this inſtance we can trace the bone in its infant ſtate, when nature had given it pliable black horny joints, that it might yield the better to the violence of the waves; but as ſoon as it became ſtronger, theſe horny black joints were no longer neceſſary, as we find the lower part of the ſtems totally overgrown with the bony ſubſtance. The furrows in this coral are deeper than thoſe of any other; inſomuch that not only the longitudinal fleſhy tubes that ſurround the bone, but even the minute pores in them, through which the *oſſeous* juice exudes, are very diſcernible ſee fig. 3.

We now come to a very ſingular circumſtance in the growth of the *gorgonia*, in which it differs remarkably from that of trees. In fig. 8. is the figure of the naked ſtem

ſtem or bone of a *gorgonia*, to which we find ſeveral tree oyſters and other ſhells have adhered. Theſe ſhell-fiſh ſeem to have killed the *gorgonia*; for the ſame ſtem ſeems to be covered over with another *gorgonia* of the ſame kind; which in its growth hath almoſt covered the ſhells, and likewiſe the branches to which they were faſtened, leaving only part of the ends of the branches of the firſt *gorgonia* yet uncovered. The ſize and weight of the ſhells probably gave the waves ſo great a power over the ſtem, that it was at laſt broken off, and caſt on ſhore in the ſtate in which it is here repreſented. This inſtance of a *gorgonia* growing over one of its own kind, ſeems ſufficient to account for the circle of calcareous matter found now and then in the croſs ſections of old ſtems, between the horny circles, as hath been obſerved by Dr. PALLAS, Elench. Zooph. p. 162. "Interjecto quandoque "tenui materiæ calcareæ ſtrato." But, I believe, no one hath ever ſeen the bark of trees incloſed in the ſame manner in the inner circles of the wood; and indeed it is ſo contrary to the laws of vegetation, that Dr. PALLAS hath not attempted to account for it, by ſhewing any parallel inſtances in the tranſverſe ſections of timber. Another inſtance of the manner of growing of theſe animals is ſtill more remarkable, ſee fig. 2. where the upper part of the *gorgonia flabellum* meeting with an obſtruction in growing upwards, has grown downwards over its own fleſhy ſubſtance, and has evidently incloſed and covered over its own reticulated branches, with a continuation of its own fleſh and bone. Dr. PALLAS, in a note on the growth of the *gorgonia*, hath the following extraor-

dinary obſervation, that a gentleman in Holland is poſſeſſed of a *gorgonia*, which has on the ſame ſhrub, the bark partly of a *gorgonia verrucoſa*, and partly of the *gorgonia coralloides*, without any viſible difference of the branches; which he accounts for by comparing it to the growth of vegetables, ſaying: "So different *lichens* are "often found incorporated in ſuch a manner together, "that they might eaſily be miſtaken for one and the ſame "plant(b). But I think it rather paradoxical to ſuppoſe the fleſh of one animal to grow on the bones of another. If he had examined it attentively, he would have found what we have advanced to be the caſe. It is not unuſual for a *gorgonia* of one ſpecies to grow upon the decayed branches of an individual of another, where the ſoft or fleſhy part is already periſhed; but the upper or living *gorgonia* muſt have its own hard as well as ſoft parts; for ſhould there be the fleſhy part, and not the bony part, it would belong to the *genus* of *alcyonium*, and occaſion ſuch another remarkable miſtake as this author has already made in his *ſertularia gorgonia*, ſee Elench. Zooph. p. 188. where he has deſcribed an *alcyonium*, growing upon and ſurrounding the ſtem and part of the branches of the *ſertularia fruteſcens*, as a new ſpecies of *ſertularia*. This, he ſays, moſt cloſely unites the *genus* of *gorgonia* with that of the *ſertularia*: and to convince me of the

(b) Quod in eodem frutice corticem partim gorgoniæ verrucoſæ partim coralloides exhibet, ſine ullo viſibili diſcrimine ramorum. Verum et diverſæ ſæpe lichenes ita ſibi invicem inoliti reperiuntur, ut pro unâ facile plantâ ſumeres. PALLAS, Elench. Zooph. p. 163.

truth

truth of what he afferts, he has fent me part of the original fpecimen, of which fig. 9. exhibits an exact reprefentation. At A is a magnified figure of this *alcyonium*, on a piece of the branch of the *fertularia*. It is of a flefhy fubftance with warts, having each twelve rays; we have many fpecies of *alcyonia* from the Weft Indies not much unlike this. The reader, by attending to the Doctor's own defcription of his *fertularia gorgonia*, will foon be convinced of the error, efpecially when he confiders, that the character of a *fertularia* is that of a branched animal, with the hard parts without, and the flefhy parts within; and that the *gorgonia*, on the contrary, hath its flefhy or foft parts without, and its bone or hard parts within.

There is another effential difference hitherto unnoticed, between the growth of the *gorgonia* and that of trees; and that is, in the connexion between the fide branches and ftem of the one, and the fide branches and ftem of the other. The fide branches of vegetables proceed from the pith; of courfe, when a ftem and fide branch is divided length ways, the pith is feen continued through the main ftem into the branch, fee fig. 10. where A. is the natural fize of a fmall twig of the lime tree, and B. the fame magnified. It muft be obferved, that in fome trees the channel or continuation of the pith which leads from the ftem to the fide branch, is very much contracted, and the communication very narrow; in which cafe it will be neceffary to make crofs fections, which will foon difcover the courfe of the pith

from

from one to another. M. DU HAMEL, an author of the firſt reputation, hath clearly demonſtrated this in his *Phyſique des Arbres*, vol. II. p. 119. tab. II. *f.* 91. Now in the *gorgonia*, the ſupport, or what is called the woody part, is indeed furniſhed with a kind of a pith or *medulla*: but when we cut the ſtem or branch through the middle lengthwiſe, we find no paſſage whatſoever between the pith of the ſtem and that of the branch, each being ſurrounded with a hard covering of its own, which hath no perforation, nor admits of any communication. Every branch of a *gorgonia* therefore hath its own pith or *medulla* peculiar to itſelf, which is never found paſſing into that of another, ſee fig. 11. A. the natural ſize, B. magnified. Again, in trees, the pith is largeſt in young ſhoots, and diſappears in old ſtems: in the *gorgonia* the *medulla* is of the ſame diameter in the old ſtems as in the young branches. In the longitudinal ſections of freſh ſhoots of trees, the pith in the microſcrope looks like a number of jointed tubes united together; and in the croſs ſections, it appears like ſo many circles. In dried ſpecimens the tubular appearance in the longitudinal ſections is more irregular; they look rather like longitudinal ranges of little tranſparent blebs, and the croſs ſections appear like circles interſecting one another in the margin; but there are many varieties of figures in the pith of different vegetables; what is mentioned here, is the common appearance of pith in moſt plants. When we cut a dry branch and ſtem of a *gorgonia* through the middle lengthwiſe, the pith appears divided into many little tranſverſe membranes, like ſmall

white *diaphragms*, ſeparated from one another about the diſtance of their own diameter. But theſe croſs membranes are found to be more numerous in ſuch as have been preſerved directly from the ſea in ſpirits; and when they are examined in the microſcope, they appear to be of the nature and ſubſtance with the *laminæ* that compoſe the horny tube that ſurround them (c).

(c) While I was comparing the longitudinal ſections of the young branche of trees with thoſe of the *gorgonia*, I was ſurprized to find ſuch a ſimilitude between the pith of a branch of a walnut tree, of a year's growth, and that of the *gorgonia*, ſee GREW, *Anat. of Plants*, tab. xix. fig. 4. A. and B.; eſpecially as we are told by a modern author, who hath publiſhed many microſcopical obſervations on the conſtruction of timber, that the cell-like diviſions in the branch of a walnut tree are only a row of ſingle blebs of pith. But the microſcope diſcovers to us, on viewing one of theſe croſs membranes, that it is compoſed of many cells ſhrunk up and united together; for, upon viewing the flat ſurface of one of them, it appeared full of circles interſecting one another, like a thick tranſverſe ſection of many other dried piths preſſed together: beſides, the thicker part of this ſhrunk-up walnut pith, all round next the inſide cloſe to the wood, when magnified, plainly ſhewed the ſame appearance of blebs as in other pith. To confirm this obſervation, May 23, 1772, I procured a young green ſhoot of a walnut tree, growing from a branch of the preceding year; and examining the pith, both in upright and tranſverſe ſections of this new ſhoot, I found that they exactly reſembled the pith of many other trees, but were full of ſap: and that the ranges of cells or blebs that occupied one of theſe ſpaces could not be leſs than a hundred, perhaps double that number of blebs. Dr. GREW takes notice, p. 120. in his *Anatomy of Plants*, that there are other trees beſide the walnut tree whoſe pith in the laſt year's ſhoot ſhrinks up and forms ſuch cavities; and an ingenious friend of mine, now engaged in an enquiry into the ſtructure of plants, hath ſhewn me a laſt year's ſtem of the *braſſica ſylveſtris*, or ſhrubby cabbage, whoſe pith is ſhrunk and divided into a ſingle row of cells, like thoſe of the walnut tree of laſt year's growth.

I come

I come now to the outſide covering or ſkin of the animal. As few have been at the pains to examine the ſurface of the *gorgonia* accurately, it hath ſcarcely yet been noticed, that they are cloathed with a kind of ſcales, and ſome of them ſo remarkably covered, and the ſcales ſo well adapted to the particular parts, that one might reaſonably be induced to think, that nature hath given them this defence, as ſhe hath done in like manner to the ſeveral parts of ſnakes and lizards, as a kind of armour to protect them from external injuries. As inſtances of the above, I ſhall only mention, that the ſurface of the ſtem as well as the mouth of the cells of the *gorgonia placomus* are defended by long pointed ſcales, ſee *Eſſay on Corallines*, p. 27. *t.* A. 1. to 3.; and the *gorgonia verticillata* (of which an elegant ſpecimen is to be ſeen in the Britiſh Muſeum) hath alſo very remarkable ſcales of different ſizes round the mouths and on the ſkin, ſee *Eſſay of Corallines*, *t.* 26. *f.* S. T. The *gorgonia lepadifera* hath a moſt remarkable variety, placed like tiles, one over another, for the defence of the mouth of the cells that incloſe the *polype* ſuckers; beſides, there is a ſmall kind of ſcales, that covers the ſurface of the ſtem and branches, ſee fig. 12.

From the ſkin we are naturally led to ſpeak of the fleſh of the *gorgonia*, or what the modern naturaliſts call the bark or *cortex*. Whoever hath examined the fleſh of the *gorgonia*, well preſerved at the ſea-ſide in ſpirits, will find, on diſſecting them, proper muſcles and tendons for extending the openings of their cells; for ſending forth

Fig.5.
Fig.8.
Fig.10.
Fig.9.
A
Fig.6.
Fig.7.


Fig. 12.
A

forth from thence their *polype* ſuckers in ſearch of food; for drawing them in ſuddenly, and contracting the *ſphincter* muſcles of theſe ſtarry cells, in order to ſecure theſe tender parts from danger; and likewiſe that there is, as we have already mentioned, proper ſecretory ducts, to furniſh and depoſit the *oſſeous* matter, for the ſupply of the bone, both of the ſtem and branches as well as the baſe, to ſecure its ſtation with firmneſs, amidſt the boiſterous element where it is appointed to be. That there are *ovaries* in theſe animals is without doubt; for in moſt of thoſe that were ſent to me preſerved in ſpirits, the eggs were very viſible upon making longitudinal ſections in the ſame manner and form as in the *alcyonium digitatum*, called *dead man's hand*, ſee Philoſophical Tranſactions, vol. LIII. tab. XX. fig. 11. but much larger; and it is very probable, many of theſe animals are *viviparous*, as we have ſeen among the *ſertulariæ*.

So that I muſt conclude, that though they grow in a branched form, they are no more allied to vegetables than they are to the ramified configurations of *ſal ammoniac*; to the elegant branched figures in the Mocha and other Gems, called *dendrites*; to the *arbor Dianæ*, or the arboreſcent figures of the Corniſh native copper: conſequently, that animal life doth not depend on bodies growing according to a certain external form. Hence it appears, that this metamorphoſis of a plant to an animal is a flowery expreſſion, and in my opinion, better ſuited to the poetical fancy of an OVID, than to that preciſe method of deſcribing which we ſo much admire in a natural hiſtorian.

 II. *The*

II. *The Variation of the Compass; containing* [(a)] 1719 *Observations to, in, and from, the* East Indies, Guinea, West Indies, *and* Mediterranean, *with the Latitudes and Longitudes at the Time of Observation. The Longitude for the most Part reckoned from the Meridian of* London; *if otherwise, it is taken Notice of in the Margin. By Mr.* Robert Douglass. *Recommended to the Public by the late Dr.* Halley. *Communicated by the Rev.* Nevil Maskelyne, *Astronomer Royal, F. R. S. with a Letter prefixed from* William Mountaine, *Esq. F. R. S. to Mr.* Maskelyne.

TO THE REV. NEVIL MASKELYNE, A. R. F. R. S.

St. John's, Southwark,
Nov. 24, 1774.

DEAR SIR,

R. June 1, 1775. MR. ROBERT DOUGLASS's observations of the variation, taken by himself, and signed by Dr. HALLEY, I now beg leave to return, with answers to the questions you proposed thereupon; *viz.* Whether I had ever seen them; or had made any use of them in my former publications? And if so, whether any further notice of them was necessary? In answer to the first; among my papers on the variation I have an exact copy of them, recommended under the same signature, of the same date, and in the same words, and the

(a) DOUGLASS calls these 1719 observations; but these tables, I rather think, should be called the Result of 1719 Observations. W. MOUNTAINE.

whole ſeems written by the ſame hand; ſo that both of them (containing 1719 obſervations each) may be ſtiled originals: I have ſent mine herewith for your inſpection. To your ſecond queſtion, I anſwer, that I could not make any uſe of them in the conſtruction of either of my charts, as the firſt was formed from actual obſervations made about the year 1744, and DOUGLASS's conclude in the year 1735; but as many of them as ſuited the periodic times were taken in, and compared with many thouſands of others, according to the order of their reſpective dates, in forming the ſix periodic reviews, as publiſhed in the Philoſophical Tranſactions, vol. L. part I. for 1757. To the third queſtion; *viz.* whether they are worth a public record? I rather think they are; and have ſometimes thought to preſent them to the Royal Society for their approbation: for, notwithſtanding they have been thus made uſe of, yet they are mixed and moulded up with others, and thoſe only to every five degrees of latitude and longitude; ſo that the variations reſpecting the ſeveral latitudes and longitudes do not ſingly appear. And as the true theory of this *arcanum* in nature is yet ſo little known, every thing that ſerves to illuſtrate it, deſerves attention. And as theſe tables, for a ſingle ſet, are extenſive, and may be eſteemed authentic, they may very well ſerve for comparatives in future times; for if the change of the variation be regulated by any conſtant law, which I ſtill continue to doubt, the diſcovery of that law muſt greatly depend upon ſuch compariſons made from multitudes of good obſervations

 taken

taken at different periods, and thofe over the whole face of the terraqueous globe; but until that law is certainly known, charts can be conftructed only from time to time from the lateft obfervations. I am, &c.

W. MOUNTAINE.

P. S. From the differences, times confidered, between thefe identical obfervations of DOUGLASS, and Dr. HALLEY'S chart in 1700, Mr. CHARLES LEADBETTER attempted, with great labour, to delineate a fet of curves for the year 1740, but without fuccefs. He feems, by his unfinifhed work, to have plunged himfelf into an inextricable labyrinth. I have the manufcript chart and his table of differences now in my cuftody. His attempt was fruftrated by the opinion he poffeffed of an uniform and regular mutation; and this failure further convinced me of the neceffity of proceeding upon other principles.

On the outſide leaf of each copy is the following teſtimonial:

Mr. ROBERT DOUGLASS *having ſhewn me this curious collection of Obſervations of the Magneticall Variation, made by himſelf in moſt parts of the world, I thought them worth preſerving, and as ſuch recommend them to the public, believing they may be of uſe both to the preſent and future navigation.*

Greenwich,
Aug. 28, 1738.

EDM. HALLEY.

To the right honourable the Lords Commiſſioners for executing the office of Lord High Admiral of Great Britain and Ireland, &c.

MY LORDS,

I HAVE already given in to this honourable board thoſe obſervations I had to the Eaſt Indies, Guinea, and Weſt Indies, but not thoſe I had in a ſecond voyage to the Weſt Indies, nor from England to the Mediterranean. Dr. HALLEY has peruſed them, and recommended them to the public. And as nothing of this nature has ever yet appeared, to my knowledge, I hope they may be of uſe for ſhips bound to Guinea, Weſt Indies, Mediterranean, and Eaſt Indies: and as I think that all things of this nature belong to this honourable board, eſpecially by me who have ſerved as ſchool-maſter many years in the royal navy; therefore I moſt humbly dedicate them to your lordſhips, and am, with all due reſpect,

YOUR LORDSHIPS

moſt humble and moſt obedient ſervant,

Jan. 30, $173\frac{8}{9}$. R[t]. DOUGLASS.

THE

THE variation of the compafs in a voyage to, in, and from, the Eaft Indies, in the years 1721, 22, 23, and part of 24, from the Ifland Madeira, on board the Lyon, THOMAS MATHEWS, Efq. Commodore. Likewife the variation to Guinea, from the Ifle of Mayo to Cape de Verd, along the Coaft to Widah, Ifland St. Thomas, Cape Lopez; back again to the Coaft of Guinea; then to the Ifland St. Thomas; from thence to Barbados, St. Chriftophers, Jamaica, Porto Bello, along the Coaft to Cartagena; then to Jamaica through the Gulph of Florrida, as far as 35° 15′ North, and Weft longitude from London 61° 56′; back again to Antegoa; from thence to England, in the years 1725, 26, and part of 27, on board the Kinfale, RICHARD GIRLINGTON, Efq. Commander. Some more to the Weft Indies, on board the Lark, JOHN GRAY, Efq. Commander, in the years 1727 and 172$\frac{7}{8}$. More from England to the Mediterranean, as far as Leghorn, on board the Dreadnought, ALEXANDER GEDDES, Efq. Commander, in the years 1730 and 31. More in the Mediterranean, as far as Tripoly in Barbary, on board the Hector, the hon. Sir ROGER BUTLER, Commander in the years 1733, 34, and 35,

Note, the firft column is the year and day of the month; the fecond and fixth, the variation; the third and feventh, the number of obfervations; the fourth, fifth, eighth, and ninth, the latitude and longitude at the time of obfervation. The longitude for the moft part reckoned

reckoned from the meridian of London; if otherwiſe, it is taken notice of in the margin. When there are more obſervations than one, that ſet down is the mean. When in ſight of land, I have ſet down its bearings and diſtance in the margin. E. ſtands for Eaſt, and W. for Weſt variation.

We obſerved with 3 compaſſes outward-bound to the Eaſt Indies. The maſter of the ſhip managed one; the pilot his own; and I had the other, which I kept the whole voyage. We had but 2 homeward-bound; they ſeldom differed ½ a degree, and not often that: they were taken with all the exactneſs poſſible. I have ſet down the latitude and longitude of moſt of the places I went from, and the latitude and longitude I made the place ly in (that I was bound to) by my reckoning.

The

The VARIATION of the COMPASS to, in, and from, the East Indies, on board the Lyon, THOMAS MATHEWS, esquire, Commodore.

1721 Mar.	Var^l. P. M.	N^r of Obs	Lat.	Long^l.	Var^n. A. M.	N^r of Obs	Lat.	Long^de.	
21	6 28W	2	29 20N	18 53W from London					Lat. of the W^t end of Madeira 32 25 N. Long^de. W^t. from London 17 42.
24	4 30	3	24 36	19 8					Bound for the island St. Jago.
25	3 36	2	23 0	19 36					
26	3 19	2	21 0	20 30					
28	2 25	2	16 40	22 38					
Apr. 8	2 30	1	14 15	23 26					Lat. of Port de Praya bay on St. Jago 15 10 N. obs^n. Long^de. W^t. from London 23 42 as I have taken it. Bound for Cape Bon Esperanza.
9	2 29	2	11 40	22 13					
10	2 21	4	9 44	21 32					
11	2 4	1	8 17	21 33					
12					2 14W	1	6 28N	20 42W	
13	2 11	2	5 40	20 37					
14	1 30	1	4 16	20 26					
15	1 20	2	3 30	20 30					
17	0 33	2	2 10	21 8	0 0	1	1 22	21 15	
18					0 40E	2	0 7	21 29	
20	0 22E	1	1 20S	21 25					

1721 Apr.	Var^n. P.M.	Nr of Obs.	Lat.	Long^de.	Var^n. A.M.	Nr of Obs.	Lat.	Long^de.	
21	0 38W	1	1 46S	21 5W					
22	1 13	2	1 48	21 0					
May 2					2 11E	2	14 10S	25 14W	
3					3 0	1	16 0	26 42	
4	3 42E	1	16 50	27 16	3 26	3	18 0	28 2	
5	4 20	3	19 0	28 22	5 15	2	20 0	28 38	
6	4 20	2	21 10	28 22	5 18	2	22 38	27 56	
7	3 56	2	23 10	27 20	4 25	2	24 30	26 27W	Bound for Cape Bon Esperanza.
9					3 0	1	26 35	23 31	
10	2 50	2	26 56	22 15					
11	1 56	2	27 7	21 26					
12	1 33	2	27 36	20 17					
13	1 8	2	29 10	17 0	1 0W	2	29 40	14 12	
15	1 31W	2	30 10	11 27					
17	3 48	3	32 40	6 20	5 23	3	33 12	4 12	
19					7 50	3	34 2	2 30E	
21	9 52	5	34 20	7 10E					
24	13 24	2	33 32	14 40	13 22	3	33 58	15 20	
25					14 12W	3	34 9	17 4	

1721 May	Var^{n}. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^{n}. A.M.	Nr of Obs.	Lat.	Long^{de}.	
26									Anch^{d} in Table Bay, at Cape Bon Esperanza. By my reckoning I make the Table Land to ly in 18 10 E. from London.
June	16 25W	11							Taken as we lay at anchor in Table Bay. I judge there is something draws the compass.
14	14 25	2							Cape Bon Esperanza S. 5 or 6 lgs.
18	19 0	3	35 20S	28 53E	20 10W	3	35 16S	30 10E	Lat. of the Cape 35 25 S: Long^{de}. E. from London 17 25.
19	20 45	4	35 14	31 12					Bound for Madagascar.
20					21 13	3	34 0	34 10	
22					22 35	3	32 2	35 38	
24					22 45	2	30 51	36 10	
25					22 40	3	30 39	36 17	
26	23 3	2	30 30	35 47	22 51	2	29 53S	36 26	
27	22 32	1	29 40	36 2					
28					22 24	2	27 33	37 12	For Madagascar.
29	23 43	2	26 38	37 34					
July 3	23 51	3	24 14	45 30					
4	24 17	4							St. Augustin's Bay on Madagascar E. ½S. 5 or 6 lgs.

1721 July	Var^n. P.M.	Nr of Obf.	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obf.	Lat.	Long^{de}.	
	23 18W	8							Taken as we lay at anchor in St. Auguftin's Bay on Madagafcar. I make St. Auguftin's Bay on Madagafcar to lye in 23 30 S. p^r obs^n. Long^{de} E^t. from London, by my reckoning, 46 21.
15	23 27	2	21 40S	45 36E	22 35W	3	21 10S	45 40E	The nearer to the land the lefs var^n.
16	23 51	2	20 47	45 57					
17	24 25	4	20 22	46 23					I take the land, called Weftminfter Hall near St. Auguftin's, to lye in 23 38 S. and E^t. long^{de} from London, 46 45
19	21 44	3	20 20						
20	22 13	4	19 48	46 8					Lat: of the mouth of Timerly river 20 20 obs^n. Long^{de} E^t. from London, by my reckoning, 46 40.
21	22 22	1	18 17	45 18					Bound for the Ifland Joanna.
22	21 25	2	17 15	45 18	21 20	1	16 25	45 6	
24	21 33	3	15 0	45 26					
25	21 16	3	13 35	46 29					
29	20 33	4							In Joanna Bay.
Aug. 2	19 12	3							Center of the Ifland Comaro, S.W. by W. 4 or 5 lgs.
3	19 44	5			19 4	3			Center of the Ifland Comaro S.S.W. 10 lgs. Center of Comaro S.$\frac{1}{4}$W. 18 lgs.

Ag.	Var^n. P.M.	Nr of Obs.	Lat.	Long^de.	Var^n. A.M.	Nr of Obs.	Lat.	Long^de.	
4					19 2W	1	10 18S	44 57E	Mean of the evening and morning. Lat. of the center of Comaro 11 44 S. longde. E. from London 45 17.
5					18 27	2	9 34	44 58	
6					18 23	2	8 28	45 10	Bound for Cape Gardafoy near the Red Sea.
7	18 33W	4	7 36S	45 24E	17 20	2	6 28	45 36	
9					16 17	2	2 2	47 55	
12	13 48	1	3 5N	51 17					
13	13 19	3	5 25	51 41					
14	11 39	2	8 45	51 11	11 14	1	9 10N	51 30	P.M. Neareſt land on the Ethiopian Coaſt W.N.W. 3 lgs. A.M. Neareſt land N.W. by N. 4 lgs.; very cold.
15					11 0	3	10 6	51 54	Neareſt land ditto Coaſt N. by W. 4 lgs.
16					11 11	3			Mount Felix at the mouth of the Red Sea W. ½ N. 5 or 6 lgs. I make Cape Gardafoy, at the mouth of the Red Sea, to ly in 11 40 N. and Et longde. from London 52 30.
18	11 49	4	12 36	53 10					I take Cape Gardafoy to ly in 11 45 N. and Et longde from London 52 50.
19	11 0	2	13 10	55 37					Bound for Bombay.
20	9 42	2	14 48	58 45					

Aug. 1721	Var^{n}. P.M.	N^{r} of Obs.	Lat.	Long^{de}.	Var^{n}. A.M.	N^{r} of Obs.	Lat.	Long^{de}.	
21	8 28W	2	16 14N	61 37E					
23	7 7	5	18 20	65 35					
24					5 35	2	19 16	68 10	
27	5 12	3							The Low Land to the S^{ord} of Valentine's Peak E^{t}. 3 lgs. near Bombay.
Sept 12	5 16	2							Taken without the W^{t} gate on the Island Bombay.
	5 13	3							Taken aboard.
Oct.	5 27	3							Taken in the road by Choul; this is about 10 or 12 lgs. to the S^{o1d} of Bombay.
18	5 19	1							Island Calaba E.S.E. 2 lgs. This island is about 8 lgs. to the S^{ord} of Bombay.
172$\frac{1}{2}$ Feb. 1	4 58	2	17 0						In sight of Rogipore Island. Bound for Goa.
4	5 49	3							Alguarda by Goa S.E. 3 or 4 lgs.
5	5 25	3							Cape Ramas near Carwar, S.E. by E. $\frac{1}{2}$ E. 3 or 4 lgs.
7	5 40	2							The W^{t}ermost point of Carwar Bay N. by W. 7 or 8 lgs.
9	5 24	2							The W^{t}ermost rocks near Manqulor N. $\frac{1}{2}$ W. 3 lgs. This place lyes to the S^{o1d} of Bastore, about 9 lgs.

172$\frac{1}{2}$

172½ Feb.	Varn. P.M.	Nr of Obf.	Lat.	Longde.	Varn. A.M.	Nr of Obf.	Lat.	Longde.	
10 11	5 35W	8							At an anchor in Manqulore Road.
13	4 21	8							In Tellicherry Road.
18	4 5	3							In Calicut Road.
19	3 34	4							Cochin Fort N. by E.½E. 4 lgs.
22	4 26	3	6 38N	77 56E					I take Anjanga to ly in 8 48N. and E. longde. from London, 77 12.
23					4 0W	6	5 24N	78 24E	Bound for the Ifland Mauritius.
24	3 24	4	4 48	78 41					
25					3 9	2	3 55	78 38	
26	3 36	5	3 24	78 34					
27	3 3	2			3 8	1			At equal altitudes, having run but 7 mile. Mean of evening and morning.
28	2 53	3	3 16	78 4	3 7	2	3 13	78 10	
Mar. 1	2 35	2	3 5	78 24	2 48	3	2 48	78 34	
2	2 16	2	2 28	78 40	3 0	3	2 0	78 47	
3	2 20	5	1 28	78 53	3 4	3	0 50	79 0	
4	2 14	4	0 28	79 9	2 8	2	0 14S	79 14	
5	1 36	2	0 54S	79 20	2 10	3	1 10	79 24	
6	2 2	2	1 20	79 30	2 16	3	1 30	79 40	

1722 Mar	Var. P.M.	N. of Obs.	Lat.	Long^{de}.	Var^{n}. A.M	N. of Obs.	Lat.	Long^{de}.	
7	1 48W	2	1 52S	79 8E					
8	2 28	2	2 45	79 36	2 46W	2	2 58S	79 36E	
9					3 6	3	4 12	79 48	
10	2 41	3	4 46	79 57	3 9	2	5 10	80 5	
11					3 24	1	5 36	80 22	
12					3 14	1	5 27	80 30	
13	2 54	2	5 27S	80 22	3 0	1	5 55	80 10	
14	3 15	2	6 26	80 2	3 28	1	7 2	79 45	
16	3 42	2	9 8	79 7					
17					5 20	2	11 36	78 30	
18	4 56	1	12 16	78 20	5 12	1	13 0	77 56	
19	5 8	2	13 50	77 20	6 15	2	15 0	76 24	
20	6 22	2	15 50	75 32	7 12	2	16 50	74 30	
21	7 26	2	17 40	73 22					
23					14 27	4	20 20	68 3	I can give no reason that the variation has risen so much. 2° 40 difference lat. and 4 19 difference of long^{de}. The var^{n}. has altered 7° 1′.
25					16 27	3	23 35	63 24	
26					17 44	1	20 41	61 6	
1722 27	17 37	4	20 42	60 57	18 47	4	20 35	59 34	

1722 Mar.	Var^n. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obs.	Lat.	Long^{de}.	
28									Between 4 and 5 P.M. saw a round island, which lies near the island Mauritius. I make this island to ly in the long^{de}. of 58 42 E. from London. I judge that this island and Don Mascareen ought to be laid down 3 degrees more to the E^{t}ard than most our books lays them in, for we often tryd the current.
Apr. 1722	18 46W	5							Taken at Peter Butts Bay, and this is about 4 mile to the N^{ord} of N.W. harbour on Mauritius.
5	18 22	1			18 56W	1			The largest of the 2 round islands near Mauritius S.W. ½ W. 4 l. A.M. d^o island W. by S. ½ S. 4 lgs.
6					19 7	1			D^o island 8 or 9 lgs.
7					19 45	3			D^o island N by W. 12 lgs. Bound for the Island Don-Mascareen.
8					19 49	2			The SE p^t of Don Mascareen W.S.W. 6 or 7 lgs.
10	19 44	1							In St. Paul's Bay at Don-Mascareen.
13	19 49	2	19 12S	52 11E					I take St. Paul's Bay to ly in 20 58 S^o. and E. long^{de}. from London 54 10. Bound for the Island St. Mary by Madagascar.
18	19 53	1							Charrock Point on Madagascar S. by W. 1 m. Long^{de}. of the N^o. end of St. Mary's 48 41, by my reckoning.

Apr. 1722	Var^n. P.M.	N^r of Obs	Lat.	Long^le.	Var^n. A.M.	N^r of Obs	Lat.	Long^l.	
19					19 52W	1			At Charnock Point on Madagascar.
22	19 25W	2				2			At anchor at the mouth of St. Mary's harbour.
May 2	18 30	2	13 34S	52 35E					Bound for Cape Amber, which is the N. E. p^t. of Madagascar.
3	19 1	5							Cape St Sebastian on the N° end Madagascar, S.W. ½ S. 4 or 5 lgs.
4	18 36	3							Cape Sebastian, E. by N. 9 lgs.
5	19 48	3	13 0		20 12	1	13 8S		The great island Nossee, E. ½ S. 5 lgs.
6	19 45	3							The Eastermost of the Isl^d Nossee, E. by N. 6 lgs.
7	19 5	3	13 34						D° Island E.N.E. 10 or 11 lgs.
8	19 20	3	13 56						We are to the So^rd of Maringambo Harbour.
9					20 16	3	14 51		To the No^rd of Soundjee point.
10					20 24	2	15 21		Near Maringambo Harbour. Mean of the evening and morning.
11	21 18	4							Mean of the morning and evening. At an anchor in the outer bay of Manigaro.
14	21 14	4							Near New Masseleage. Mean of the evening and morning.

1722 May	Var^n. P.M.	N^r of Obs.	Lat.	Long^de.	Var^n. A.M.	N^r of Obs.	Lat.	Long^de.	
15	21 37W	2							Jamgomy Island S. by W. 5 lgs. Going back to Manigaro Harbour.
16	20 51	4							Near Manigaro Harbour.
18 to 21	21 22	7							At an anchor in the inner bay of Manigaro.
June 7	21 43	1			21 14W	2	14 37	46 40E	Mackamoy Ild S.W. by S. 7 lgs. Bound for the Island Joanna. Lat. of the N.W. part of Manigaro Harb. 15 45 S^o obs^n. Long^de. E. from London 47 26.
8	21 12	2			20 24	1		-	Saw Valentine's Peak on the Island Mayo a N.NE. 14 lgs. Long^de. 45 46 E. Valentine's Peak N. by E. 6 leagues.
12	20 39 20 33	2 2							At an anchor in Joanna Bay, and with the same compass I observed here in June last. By another compass.
18	19 18	3	9 54S	45 34E	18 54	3	8 47	45 23	Lat. of the N.E^t. part of Joanna 12 2 S. Long^de. E^t. from London 46 0, as I take it.
19	18 11	3	7 48	45 14					Bound to Fort St. George by the N^o. end of the Maldivee Islands.
20	16 48	1	5 24	45 0					
21	16 15	2	2 26	45 56					
22	15 34	3	0 35	48 21					
23	15 4	2	0 18N	51 4					

1722 June	Var^n. P.M.	N^r of Obs.	Lat.	Long^{de}.	Var^n. A.M.	N^r of Obs.	Lat.	Long^{de}.	
24	13 59W	2	2 10N	54 10E					
25	12 43	3	3 16	57 2					
27	10 38	2	5 36	62 4					
29	9 11	2	8 34	66 45					
30	7 6	5	7 59	69 50					
July 1	7 8	4	7 53	71 44					
2	7 6	3	7 46	72 16	6 45W	1	7 42N	72 35E	
4	5 54	5	7 15	74 6	5 32	2	7 13	74 40	
5	4 56	3	7 10	75 22	5 0	1	7 0	76 10	
6	4 7	4			4 25	1			At equal altitudes evening and morning. In sight of the N°. end of the Maldivees. I make the N°ermost of the Maldivee Islands to ly in 7 12 N. by a good obs^n. and I make it ly in 77 15 E. from London. A fair wind all the way; but this is about 4° to the E^{t}ard of what our books lays it in; and its my opinion, that Mauritius and Don-Mascareen, and the N°. end of Maldivees ought to be laid down about 3° further to the E^{t}ard than they are.
7					4 43	3	7 5	0 49E^t from the N°most Maldivees.	
9	2 52	5	5 38	4 12E from the N°most of the Maldivee Islands.					

1722 July	Var^{n}. P.M.	Nr of Obf.	Lat.	Long^{de}.	Var^{n}. A.M.	Nr of Obf.	Lat.	Long^{de}.	
10					2 13W	3	5 49N	7 43E from the N^{o}moft of the Maldivees	A little after obferv^{n}. faw the Ifland of Zeloan. Note, I have made 7 56 long^{de} between the N^{o} end of the Maldivees and the land called the Elephant on Zeloan.
11					2 12	1			Fryers Hood on the S.E. part of Zeloan W.S.W. 8 lgs.
12	2 15W	1	8 30N		2 36	2	10 2	10 48W from Fryers Hood on Zeloan.	
July	2 52	9							At Fort St. George or Maderafs.
29	1 42	2	16 20	3 38E from Fort St. George.	1 32	1	17 0	4 16E	Bound for Balafore. The nearer the land I find the lefs variation.
30	1 38	1	18 3	4 41E from Fort St. George.					Note, I have made 5° 5′ long^{de}. between Fort St. George and Jackernat Pagoda.
Aug. 7ber 8ber	3 4	26							At an anchor before the mouth of Rogues river; and, as I was informed, this is about 4 lgs. from the mouth of the Ganges.
28	3 33	2							Cape Palmiras near Palafore W. ½ N. 4 lgs. Bound for Surrat.
29	3 0	1	18 56	0 9W from Cape Palmiras.					
31	3 17	2	14 50	2 5W from ditto.					The compaffes new touched.

1722

1722 9bre	Var^n. P.M.	N^r of Obs.	Lat.	Long^de.	Var^n. A.M.	N^r of Obs.	Lat.	Long^de.	
1	2 14W	1	12 53N	2 54W	2 2W	2	12 0N	3 17W^t from Palmiras.	
2	2 48	2	11 12	3 35					
4	2 21	2							Fryers Hood on Zeloan S W½ W 8 lgs. Note, I have made 4° 38′ longde. between Cape Palmiras and Fryers Hood on Zeloan.
5	2 36	1	5 44	0 23W from Fryers Hood.					
11	4 11	3	9 14						Near Porcat, a Dutch factory on the coast of Malabar.
12	4 13	3							The ships in Cochin road N. by E. 4 or 5 lgs.
13	3 34	3							Cochin Fort E.N.E. 2 lgs.
14	3 14	4							We are about midway between Pansan Factory and Calicut.
15	4 9	3							In Calicut Road.
16	3 44	3							Sacrifice Rock S.S.W. ½ W. 4 mile. This rock is about 4 or 5 lgs. to the Sord of Tellicherry.
19	4 4	3							Tellecherry Fort S. E. by E. 5 lgs.
21	5 20	3							Saw Pidgeon Island from the mast-head N. by W. 10 lgs. Mallapy Hill E.N.E^t.
22	4 43	2							Pidgeon Island S. by E. 6 lgs.

1722 9ber	Var^n. P. M.	Nr of Obs.	Lat.	Long^de.	Var^n. A. M.	Nr of Obs.	Lat.	Long^de.	
23	5 4W	3							In Carwar Bay.
25	4 37	4							Carwar Bay S.E. by S. ½E. 4 lgs.
26 27 28	4 57	8							In Goa Road.
30	4 56	2	16 6N						About 8 lgs. to the Nord of Vingorla Rocks.
xber. 1	4 47	2	16 24						The mouth of Careptam Harbour E. by S. 3 mile.
2	4 50	2	16 50						Cape Dobs N.N.E. 5 lgs.
4	4 43	2	17 45						Sevendruke N.E. by E. 3 lgs. This is a rock fortified by Angria, and is about 4 lgs. to the Nord of Setra River.
5	4 50	3	17 55						Neareſt land E.N.E. 6 lgs.
6	5 16	3							Iſland Canary near Bombay N. ½ E. 7 lgs.
9	5 28	2							The land called the Neats Tongue E. by S. 8 lgs. Bound for Surrat.
10	5 36	1	19 37						Neareſt land E. 5 lgs. Between Bombay and St. Johns.
11	5 27	3							The high land of St. Johns S.S.E. 5 lgs. from the neareſt land.
12 to 20	5 50	17							In Surrat Road.

1722

1722 xber.	Var^n. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obs.	Lat.	Long^{de}.	
22	5 23W	2							Demaen E S.E. 4 or 5 lgs. Bound for Bombay
23	5 26	2							The Neats Tongue S.E. by E. 7 lgs.
29 to 31	5 7	6							At Bombay.
Feb. 5 to 9	5 59	7							In Surrat Road.
172$\frac{2}{3}$ 14	5 30	1							We are about 7 or 8 lgs. to the Sord of Dundee Rogipore. Bound for Goa.
15	5 5	1	16 14N						We are a little to the Sord of Careptam Harbour.
17 to 19	5 13	5							In Goa Road.
24	5 8	3							In Carwar Bay.
25	5 5	1							The Soermoſt part of Carwar Bay N. ½ W. 5 lgs.
27 28 Mar. 1	5 5	8							In Mangulore Road.
3 4	4 9	3							In Tellecherry Road.
7	4 22	2	11 18	75 30E from London.					I take Sacrifice Rock to ly in 11 30 N. and Et. longde from London 75 38.
8	4 51	3	10 35	74 32					Bound for the Iſland Saccatra.

$1722\frac{2}{3}$ Mar.	Var^n. P.M.	Nr of Obs.	Lat.	Long^de.	Var^n. A.M.	Nr of Obs.	Lat.	Long^de.	
9	5 11W	2	9 54N	72 55E					Island Qualpenna 10 0 N. obsn. Longde. Et. from London 73 20.
10	5 24	1	9 43	72 25					
11	5 46	1	10 6	72 21					
13	6 3	3	9 54	71 32					
14	5 43	2	9 46	71 17					
15	5 44	1	10 0	71 17					
16	5 38	1	9 38	70 55					
19	7 14	2	9 12	68 5	7 0W	1	9 25N	67 29E from London	
20	7 11	2	9 25	67 10	7 58	1	9 40	66 40	
21	8 30	2	9 45	66 9	8 22	1	9 54	65 28	
22	8 42	1	10 0	64 55					
23	9 16	2	10 37	63 53	8 33	2	11 12	63 27	
24	9 28	3	11 20	63 0	9 24	2	11 36	62 30	
25	9 54	4	11 40	62 3	9 45	2	12 0	61 20	
1723 26	10 12	2	12 10	60 53	9 48	1	12 20	60 11	
7					9 41	2	1	59 21	
28	*				9 48	2	12 21	59 30	

1723 Mar	Var^{n}. P.M.	N. of Obs.	Lat.	Long^{de}.	Var^{n}. A.M.	N. of Obs.	Lat.	Long^{de}.	
29	9 16W	1	12 30N	59 30E	9 18W	4	12 22N	59 20E from London.	
30	9 29	2	12 28	59 17	10 8	1	12 23	59 13	
31	9 48	1	12 32	59 12					
Apr. 1					10 6	1	12 45	58 19	
2	10 40	1	12 50	57 40	10 39	2	12 50	56 57	
3	10 45	2	12 48	56 0	11 26	1	12 44	54 45	
4	11 16	2	12 36		11 33	1			The E^{t}. end of the island Saccatra W. ½ N. 9 lgs.
5	11 36	1							E^{t} end of Saccatra S.E. by S. 5 lgs. Note, I have made 21° 8′ of long^{de}. between the island Qualpenna and the E^{t}. end of Saccatra; but as those places is laid down in books, I should not have made so much by about 2°. Lat. of the E^{t}most point of Saccatra 12 20 N. obs.
8	11 38	4							At an anchor before the town of Saccatra, which stands on the N^{o}. side of the island.
10	12 4	1							Lat. of the town 12 20 N. obs^{n}. The W^{t}. end of Saccatra S.W. ½ W. 5 lgs.
11	11 54 12 0	2							The mean between the morning and evening. At equal altitudes morning and evening.

1723

1723 Apr.	Var^n. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obs.	Lat.	Long^{de}.	
12	12 43W	2							The E^t. end of the island Abdelhura S. by E. 10 lgs.
13	12 18	2							Mount Feelix E^t S^{o}erly 5 lgs.
14	12 16	2	11 36N	1 0W from Mount Feelix.					
15	12 58	1	11 25	2 15	12 32W	1	11 46N	3 6W from Mount Feelix.	
16					13 4	2	12 36	4 38W from D^o.	
17	12 37	3	12 50	4 48W from D^o.					
18	13 50	2			13 40	2	12 36	0 56W from Cape Aden.	Cape Aden N.E. ½ N. 5 lgs. This Cape ought to be laid down a degree more to the W^{t}ward than it is.
19	14 20	1							Taken in the narrow straits of Babelmandel.
20 to 27	13 34	7							In Mocha Road in the Red Sea.
May 14	14 1	1							Zee Hill near the narrow straits of Babelmandell E. by S. ½ S. 2 lgs.
15	14 8	1							The middle of the narrow straits of Babelmandell S. by E. 2 lgs.

1723 May	Var^n. P.M.	N^r of Obs.	Lat.	Long^{de}.	Var^n. A.M.	N^r of Obs.	Lat.	Long^{de}.	
19	13 54W	1							The higheft land of Cape Aden N.W. ½ W. 5 lgs.
20	13 42	1							Cape Aden W.N.W. ½ N. 9 lgs.
21	13 25	2	12 32N	0 30E from Cape Aden.					
22	13 10	1	12 16	1 0E from D^o.					
23	13 6	1	11 56	2 4E from D^o.					
24	13 23	2	11 58	2 30					
25	12 39	2	12 4	3 12					
27	13 3	3							Within 5 or 6 lgs. of the Ethiopian fhoar.
28	12 0	2							About 3 or 4 lgs. from Mount Feelix. Note, I made 6° 14' long^{de} from Mount Feelix to Cape Aden, and in coming back 6° 21.
29	12 34	2							Cape Gardafoy S.E. ½ E. 5 lgs.
31	12 20	2							The town of Saccatra S.W. by W. 6 lgs.
June 4	8 26	1	9 50	66 50E from London.					I take the E^t. end of the ifland Saccatra in 12 20 N. obs^n. and E^t. long^{de}. from London 56 22.

1723 June	Var^n. P. M.	N^r of Obs.	Lat.	Long^de.	Var^n. A. M.	N^r of Obs.	Lat.	Long^de.	
5	7 34W	2	8 40N	69 14E					Bound for Fort St. David's on the Coast of Cormandel by the N°. end of the Maldivee Islands. Note, I have made 20° 38′ long^de. between the E^t. end of the Island Saccatra and the N°rmost of the Maldivee Islands; and this makes the N°most island to ly in 77 0 E. from London; and in sailing from the Island Joanna to this N°most island I made it ly in 77 15. Note, we had a fair wind both voyages.
11	2 2	2	5 53	7 4E from the N°most of the Maldivee Islands.					In the morning saw the Island Zeloan to the W^t ward of Dondra-Head. of long^de. between the N°most of the Maldivee Islands and Dondra-head on the Island Zeloan 7° 46′. Fair wind all the way.
29	2 25	4							At Fort St. Davids. Mean of the evening and morning.
July 15 17	3 16	4							At Fort St. George or Maderass.
8ber 14	2 6	3			2 8W	1	5 23N		Dondra Head (N.E.½E. 6 lgs.) on Zeloan. In sight of Zeloan.
15	2 46	1							The Dutch factory at Point de Gallee on Zeloan N.N.E. 2 lgs.
16	2 45	2							A land that appears like a haycock near P^t. de Gallee on Zeloan E. ½ S°. About 6 lgs. from the nearest land.

1723 8ber.	Varn. P.M.	Nr of Obs.	Lat.	Longde.	Varn. A.M.	Nr of Obs.	Lat.	Longde.	
17	2 44W	1	7 20N	0 53W from Point de Gallee.	2 52W	1	7 54N	1 22W from Point de Gallee on Zeloan	Bound for Bombay.
18	2 51	3			2 48	3			The high land of Cape Cameroon W. 10 lgs. Cape Cameroon W. by N. ½N. 10 lgs.
19	2 50	1							The high land over Cape Cameroon, W.N.W. 6 lgs.
20	3 9	1							Cape Comaroon N.N.W. ½W. 3 or 4 lgs.
21	2 58	2							Do. Cape Et. 4 or 5 lgs.
9ber 1	5 8	2							Manquloie E. by N. ½N. 3 or 4 lgs. On the Malabar Coast.
2	5 13	3							Bassalore Hill N.E. We are about 5 lgs. from the nearest land.
3	4 48	2							The Southermost part of Carwar Bay N. 6 lgs.
4	5 48	4							Cape Ramas E. by N. 2 lgs. Here there is something that draws the compass.
6	5 3	4							At Goa.
9	5 26	3							Rogipore N.E. ½E. 5 or 6 lgs.
10	5 28	2							Cape Dobbs E. by N. 2 lgs.
12	5 25	3							Island Canary near Bombay N. by E. ½E. 6 lgs.

1723 9ber	Var^{n}. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^{n}. A.M.	Nr of Obs.	Lat.	Long^{de}.	
13	5 10W	2							Going into Bombay.
xber 17 18	5 48	4							In Surrat Road.
xber	5 22	1							At Bombay.
1723¾ Jan.	5 41	3							At Goa.
5	5 32	2							In Carwar Bay.
6	5 33	2							Pidgeon Island S.E. by S. 3 lgs
9	2 46	1							Mount Dilla near Tellecherry E N.E. 2 mile. Here its plain that being near the land the compass is drawn.
10 11 12	4 34	6							In Tellecherry Road.
16	3 41	1							Note, I have all along observed that the variation decreases after you pass Tellecherry, till you are 4 or 5 lgs. to the S^{ord}. of Cochin; then increases again till you are to the S^{ord} of Anjanga.
18 19	3 26	2							In Cochin Road.
21	4 16	1							Cochin Road N.E. ½ E. 4 or 5 lgs.
22	4 38	1	9 20N						About midway between Cochin and Anjanga.

1724 Mar.	Var^n. P.M.	N. of Obs.	Lat.	Long^de.	Var^n. A.M.	N. of Obs.	Lat.	Long^de.	
12	18 43W	4	36 40S	22 50E	18 56W	2	36 10S	21 38E	
13	18 16	2	35 57	20 47					
14	17 3	6	36 8	18 24	16 53	3	36 6	18 4	
15	16 23	3	36 6	17 53					
16	16 12	4							Cape Falso near Cape Bon-Esperanza N.W. 6 lgs.
21 to 29	16 27	8							In Table Bay at Cape Bon-Esperanza. The fort S.S.W. 2 mile.
26 to 29	16 18	5							By another compass.
31	16 15	2							Going out of Table Bay. The fort S.S.W.½W. 6 or 7 mile.
Apr. 1	15 8	2	32 47	15 36					Lat. of the Table land 34 16 So. Long^de E. from London 17 24.
2	15 15	2	32 44	14 50					
3	14 59	1	32 0	14 36	14 51	2	31 26	14 27	
4	14 33	3	31 20	14 34					
5					14 27	3	29 31	13 55	
6	14 30	6	29 0	13 34					Bound for St. Helena.
7	13 0	2	27 15	11 22					
9	11 40	2	23 8	6 29	12 4	4	22 20	5 37	

1724 Apr.	Varn. P.M.	Nr of Obs.	Lat.	Longde.	Varn. A.M.	Nr of Obs.	Lat.	Longde.	
10	12 2W	6	21 56S	5 0E					
13	10 47	2	17 54	1 0	10 10W	2	16 55S	0 8W	
14					9 30	2	16 4	2 48	
15					8 6	1	15 54	4 47	
16	7 18	3							The center of the island Wt. Noerly 6 or 7 lgs. I make St. Helena to ly in 6 16 W. from London, by my reckoning.
17 to 26	7 42	5							In St. Helena Road.
24	7 26	2							By another compass.
30	7 22	1							St. Helena Fort S.E. 3 lgs.
May 1	7 30	1	14 36	7 32W from London.					I take St. Helena Fort to ly in 16 0 So. and Wt. Longde from London 6 5.
2	6 54	4	13 26	8 36					Bound for England.
4	5 44	4	10 36	11 6					
5	5 27	6	9 4	12 24					
6	5 1	2	7 18	13 55					
7	4 37	2	5 30	15 21					
8	4 46	1	3 46	16 17					
9	4 16	1	2 6	16 50	3 48	1	1 14	17 8	
11	3 40	1	1 0N	17 40	3 39		2 9N	17 49	

1724 May	Var^n. P.M.	N. of Obs.	Lat.	Long^de.	Var^n. A.M.	N. of Obs.	Lat.	Long^de.	
12	3 34W	1	3 8N	17 57W	2 56W	2	4 6N	18 5W	
16					3 29	2	9 28	18 57	
17	3 55	4	9 46	18 53					
18	4 26	1	10 20	9 31					Bound for England.
19					3 18	1	11 18	20 40W from London.	
20	3 55	3	11 39	21 13	3 38	2	11 30	21 46	
21	3 38	2	11 28	22 16					
23	2 26	1	12 0	23 59					
24	2 45	3	13 8	25 1					
25					2 15	2	14 30	26 29	
27	2 46	3	16 0	28 10					
28	2 8	5	17 23	29 17					
29	2 43	2	18 42	29 22					
30	3 26	4	19 58	31 14					
31	3 12	6	21 25	31 41					
June 1	2 48	6	22 25	32 15					
2					3 29	3	25 5	32 8	

1724 June	Var^{n}. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^{n}. A.M.	Nr of Obs.	Lat.	Long^{de}.	
3	3 39W	3	25 44N	33 30W					
4	4 22	2	26 31	34 23					
5	5 8	5	27 18	35 5	4 37W	2	27 43N	35 29W	
6	5 15	4	28 6	36 0					
7	5 30	3	28 56	36 22					
8	5 41	3	29 16	36 23					
9					5 42	1	30 24	36 40	
10	6 10	5	30 46	36 52	5 58	2	31 14	37 15	
12	6 57	4	32 26	38 13					
13	7 15	2	32 58	39 3					
15	7 19	4	34 30	40 30					
16	7 47	4	35 46	41 14					
17	8 29	4	37 0	41 39					
18	8 55	3	38 40	42 4					
19	8 21	5	39 33	40 37					
20	8 52	4	40 42	38 37					
21	9 16	3	41 43	36 5					
22	9 37	3	42 26	33 41					
23	9 38	2	43 34	31 7					

1724 June	Var^n. P.M.	Nr of Obſ	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obſ	Lat.	Long^{de}.	
24	10 38W	6	44 16N	28 26W					
25	11 28	1	44 30	28 5					
29	11 14	2	45 26	27 22					
30	11 50	4	46 0	25 18					
July 1	11 55	6	46 34	23 14					
2	11 51	1	46 54	21 54					
3	12 10	1	47 32	19 33					

Note, The Bill of Portland was the firſt land we made, and I am about $\frac{1}{2}$ a degree further to the E^{t}ard then it is laid down in our books.

The VARIATION of the COMPASS in a voyage to Guinea, Wt. Indies, and back to England, in the Kinsale.

1725 8ber.	Varn. P. M.	Nr of Obs.	Lat.	Longde.	Varn. A. M.	Nr of Obs.	Lat.	Longde.	
5	3 32W	1							The Peakt Hill on the Isle of Mayo S.S.E.; we are about a mile from the shore.
7 8	4 5	4							At anchor in Port de Prya Bay on the S.Et. part of the Island St. Jago.
11	4 36	2	14 0N	20 0Wt from London.					Bound for Cape de Verd, Lat. of Port de Prya Bay 15 10N. obsn. Longde. Wt. from London 23 42.
12	5 4	2	13 33	18 36					
13	5 21	3	13 44	17 30					At noon saw Cape de Verd N.N.E. 3 lgs.
14	5 11	1	13 53						In sight of Gambia River.
15	6 29	2							Cape St. Marys near Gambia River S.W. 5 or 6 lgs.
to 21	6 19	8							At a anchor in the mouth of Gambia River.
25	4 36	1	11 6	16 12	4 59W	2	10 42N	15 54W	Lat. of Cape St. Marys 13 20 N. Longde. Wt. from London 15 36.
26					4 54		10 18	15 20	Bound for Siera Leon.
29	4 18	1	9 52	14 54	3 50		9 28	14 36	
30	4 13	2	9 12	14 16					

1725 9ber	Var^n. P.M.	Nr of Obs.	Lat.	Long^dr.	Var^n. A.M.	Nr of Obs.	Lat.	Long^de.	
1	3 49W	2	9 16N	14 10W					
8 9	5 12	3							In Siera Leon River I judge there is something draws the compass.
14	4 47	2	7 56	12 42					Lat of Bonas Islands 8 6 N. Long^de Wt. from London 12 10
15	4 52	1	7 59	12 48					Bound for Cape Mount
19	5 49	2	7 6	11 50					
20	6 20	2	6 56	11 9					
21					5 48W	1	6 42N	10 49W	
22	5 24	2	6 42	10 49					By equal altitudes evening and morning
24	7 0	2			7 5	1			Cape Mount E S E 5 or 6 lgs. By equal altitudes evening and morning
25	6 58	2							Cape Mount E N.E 2 mile.
27	7 15	2							Near the River St. John, and about 3 or 4 mile from the shoar.
28					7 30	1			Near Great Bassa, and a little to the Wtd. of the River St. John.
29	7 28	2							Great Bassa Nb. 3 lgs. 2 miles from the shoar.
30	7 46	1							About 4 lgs to the Wtard of Sestor or Sestos Road.

1725 8ber	Var^n. P.M.	N^r of Obs.	Lat.	Long^de.	Var^n. A.M.	N^r of Obs.	Lat.	Long^de.	
4	7 30W	2							In Sestor Road.
5	7 15	2							The mouth of Sestor River N.E. by N. 4 lgs.
7	9 8	1							Setra Crue N. by W. 3 lgs.
11	9 38	2							About 5 lgs. to the W^t ward of the river St. Andrews.
13	9 54	1							About 3 lgs. to the W^t ward of Cape de la Hou.
15	10 58	1							River Casto N.W.byN. 3 mile.
18	11 0	1							The Village Tabo N.N.E.½E. 3 or 4 mile.
20 22 24	11 37	5							At Axim.
Jan. 1	11 55	3			12 10W	1			At Cape Coast. At equal altitudes.
5	11 25	3							The English fort at Accara N.W. 2 mile.
7	12 0	1							A little to the W^t ward of the River Volta.
8	12 17	2							The flag at Little Papa N.byE. ½E. 2 mile.
11	12 45	2							In Widah Road.
20	13 28	1	4 50	2 23E from London.					Lat. of Widah 6 40N. Long^de. E^t. from London 2° 10′.

$1725\frac{5}{6}$ Jan.	Var^n. P.M.	Nr of Obf.	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obf.	Lat.	Long^{de}.	
21	13 14W	1	3 42N	2 35E	13 30W	1	3 25N	2 32E^t from London.	Bound for the Ifl^d St. Thomas.
23	13 54	3	2 16	3 30	13 50	1	2 2	3 40	
24	14 7	3	1 48	3 58					
25	14 19	1	1 31	4 53	14 40	2	1 32	5 15	
26	14 36	1	1 27	4 56					
31	14 48	1							At the Ifland St. Thomas. I make the Ifl^d Anna de Chary by St. Thomas to ly in 00 16 N. per obs^n. and E^t. long^{de}. from London 6 16.
Feb. 8	14 44	1							The Ifland Anna de Chary near the Ifland St. Thomas S.E. by E. about 7 lgs.
10					14 56	1	1 16S	1 16E from the Ifland Anna-bona.	Bound for Cape Lopoz.
11	14 35	1	1 10S	1 44E from Anna-bona.					
15					14 50	1	0 56	0 56W from Cape Lopoz.	
16	14 30	1	1 4	1 17W from Cape Lopoz.	15 1	2	1 9	1 43	
20	14 21	1	1 30	1 4W from Annabona.					Lat. of the Ifland Anna-bona 1 48 S^o. obs^n.

1726 Feb.	Varn. P.M.	Nr of Obs.	Lat.	Long.	A.M. Varn.	Nr of Obs.	Lat.	Longde.	
22	13 10W	2	1 46E	2 11W from Do.	13 48W	1	1 38S	2 15W from Anabon.	Bound for the Coast of Guinea again.
23	13 28	2	1 35	2 19	13 37	2	1 36	2 41	
24	13 46	1	1 39	3 10	13 50	1	1 25	3 35	
27	12 20	2	0 50	7 24	12 6	2	0 40	8 4	
28	12 21	2	0 20	8 18	12 25	1	0	8 35	
Mar 1	12 6	1	0 24N	8 55	12 20	1	1 10N	9 23	
2	12 6	1	1 34	9 42	11 44	2	1 50	9 57	
3	11 47	2	2 4	10 3					
5	11 30	3	4 16	11 21					
6 7	11 50 11 40	2 1							3 equal Altitudes.
11	11 40	1							At Dicky's Cove.
14	11 27	2							Little Commendo N. by E. ½E. 4 lgs.
15	11 46	2							At Cape Coast.
21	11 34	1							At Annamaboe.
23 24	11 53	1							At Accra. Mean between the 3 last and this one.
28	12 32	2							At an anchor about 5 lgs. to the Wtward of Litle Papa.
29	12 43	2							At Litle Papa.

1726 Apr.	Var^n. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obs.	Lat.	Long^{de}.	
13 14 17	13 10W	4							In Widah Road.
22	13 0	1	5 18N	2 46E from London.					Lat. of Widah 6 48 N. obs^n. Long^{de}. E^t. from London 2 10.
24	13 11	1	4 21	2 50					
30	14 35 14 29*	2							At the Island St. Thomas. *By equal altitudes evening and morning. Note, by the of long^{de}. made from Widah this and the last time, I differed but 4 minutes; so that I judge the Island St. Thomas is laid down almost 2 degrees too far E^{t}erly, and our passage from St. Thomas to Barbadoes confirms me in this oppinion, having gained that passage in 33 days, and had a fair wind all the way.
May 19	13 44	1	0 4	3 48E from London.	13 44W	1	0 10S	2 48E from London.	Lat. of the Island Anna de Chares by St. Thom. 0 16N. Long^{de}. E^t. from London 6 16.
20	13 10	2	0 28S	1 51	12 42	1	0 46	0 39	Bound for Barbadoes.
21	12 34	1	1 2	0 13W from London.	11 38	1	1 18	1 33W from London.	
22	11 12	1	1 33	2 41					
23	10 4	1	1 45	5 15					
24	8 35	1	2 9	7 46	8 34	1	2 22	9 32	

1726 May	Varn. P.M.	Nr of Obs	Lat.	Longde.	Varn. A.M.	Nr of Obs	Lat.	Longde.	
25	6 54W	1	2 31S	10 44W	6 52W	1	2 36S	11 51W	
26	6 23	2	2 40	12 40					
27	4 53	2	2 38	14 30	4 24	2	2 29	15 34	
28	3 31	2	2 30	16 24	2 21	2	2 50	17 36	
29	2 25	1	2 52	18 39					
30					0 41	2	2 53	22 33	
31	0 5E	1	2 46	23 44	0 4E	2	2 27	25 1	
June 1	0 17W	2	2 12	26 6	0 16W	3	1 33	27 2	
2	0 16E	2	1 0	28 4	0 18E	3	0 19	29 7	
3	0 22	1	0 25N	29 40	0 19	1	1 7N	30 39	
4	0 36	2	1 38	31 18	0 26	1	2 20	31 55	
6					0 26	1	4 20	34 6	
7	0 34	1	4 42	4 13	0 37	2	5 0	34 16	
8	0 30	2	5 20	34 22W					Bound for Barbadoes.
9	0 14	1	6 47	35 33	0 10	1	7 4	37 0Wt. from London.	
11					0 36	1	8 42	41 10	
13	0 50	1	10 4	44 52	1 32	1	10 48	46 25	
14	1 36	1	11 18	47 43	1 26	1	11 48	49 2	
15					2 22	1	12 26	51 6	

1726 June	Var^{n}. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^{n}. A.M.	Nr of Obs.	Lat.	Long^{de}.	
16	2 16E	1	12 44N	52 22W	2 37E	2	12 56N	54 6W	
17	3 0	1	13 5	55 28	3 36	1	13 12	57 3	Note, I make the center of Barbadoes to ly in 13 27 N. by obs^{n}. and W^{t}. long^{de}. from London 58 1'. Having a fair wind all the way, this makes me believe that the Island Anna de Chares; viz. 16 16 E. is near the truth.
26 27 28	4 24	3							In Carlisle Bay at Barbadoes.
29	3 26	1							Lambert's Point on the N.W^{t}. part of Barbadoes E.S.E. 5 lgs.
July 1	3 22	1							The W^{t}ermost part of Guardalupa N.E. ½ N. 4 or 5 lgs.
2	4 10	1							At Backstar on the Island St. Christophers.
6	5 22	1	17 58	9 2W from Palmeta Point on the Island St. Christophers.					Within 4 or 5 lg^{s}. of Hispaniola, and a little to the W^{t}ward of Cape Zapazen on Hispaniola.
7	5 12	1	17 40	0 46W from the Island Alovalla near Cape Alongia on Hispaniola.					

1726

1726 July	Var^n. P. M.	Nr of Obs.	Lat.	Long^de.	Var^n. A. M.	Nr of Obs.	Lat.	Long^de.	
9	5 3E	1	18 0N	0 46W from D°.	5 12E	1			Saw the E^t. end of Jamaica.
12	6 2	1	17 6	0 12E from Portland Point on Jamaica					Bound for Porto Bell.
13					6 30	1	14 28N	0 38W from Portland Point on Jamaica	
14	7 0	1	13 32	0 58W from D°.	7 8	1	12 27	1 25W from D°.	
15	7 16	1	11 32	1 52W from D°	7 30	1	10 46	2 18W from D°.	
16					7 22	1	9 51	2 39W from D°.	
22					7 48	1			At an anchor without the Island called the Great Bastimentos.
Aug. 7	7 24	1							Cape Cathivas near the Gulph of Darien S.E. by S. 7 lgs.
9	7 30	1	9 40	1 27E from Cape Cathivas.					
11	6 50	1							The land called Mother la Pope near Cartagena S. by E. ½ E. 5 lgs.

1726 Aug.	Var^n. P.M.	Nr of Obs.	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obs.	Lat.	Long^{de}.	
12	6 20E	1							Point Jamba near Cartagena E.S.E. 3 lgs.
13	6 14	1	12 52N	0 42W^t from Point Jamba.					Bound for Jamaica.
14	6 4	1	14 0	0 50W from D^o.					
15	5 42	1	15 18	1 3W from D^o.					
16					5 28E	2	17 6N	1 30W^t from D^o.	
7ber 12	4 31	3							At Port Royal in Jamaica.
8ber 12					4 49	1	18 40	1 53W^t from Point Negril on Jamaica.	
13					4 41	1	19 54	0 57W^t from the N.W. end of the Grand Commanis	
14					4 4	1	20 44	2 7W^t from D^o.	Bound through the Gulph of Florida.
17	4 7	2	22 53	0 16W^t from Cape St. Antonio on the W^t. end of the Island Cuba.					

1726 8ber	Var^n. P. M.	Nr of Obf.	Lat.	Long^{de}.	Var^n. A. M.	Nr of Obf.	Lat.	Long^{de}.	
18	4 25E	2	22 31N	0 55E^t from Cape Antonio.					
19					4 0E	1			The middle of the Table Land S. by E. ½ E. 4 lgs. This Table Land is about 5 lgs. to the W^{t}ward of the Havanna.
23					4 24	1			The high round hill, called Pan de Mathaneas, a little to the E^{t}ward of the bay of the fame name on Cuba, bore S. W. by S. 10 or 12 lgs.
24	4 23	2	23 30	0 38E from Pan de Mathaneas.					
25	3 26	1							The land about Cape Florida N. by W. 6 lgs. A great fwell.
26	3 5	1	25 12	81 20W from London.					In the Gulph. Lat. of Cape Florida 24 48 N. Long^{de}. W^t. from London 21 55
28					2 11	1	27 50N	81 30W from London.	A great fwell.
30					0 56	1	29 35	80 40	D^o. fwell.
9ber 3					2 14W	1	33 25	71 52	A very great fea.
6					5 12	1	35 4	64 6	A great fea.
8	5 8W	1	35 15	61 56					

1726 9ber	Var^n. P.M.	N^r of Obſ.	Lat.	Long^de.	Var^n. A.M.	N^r of Obſ.	Lat.	Long^de.	
9	4 55W	1	35 0N	61 55W from London.					Going back to the W^t. Indies. Swell much abated.
10	5 6	1	33 14	61 14					
14	0 41	2	26 14	58 9	0 36W	1	25 48N	58 6W from London	
15	1 3	1	25 41	58 0	0 54	1	25 20	37 14	
16	4	1	24 48	56 38					
17	0 20	1	23 0	56 35					
18	0 32E	1	21 7	56 32	1 46E	1	19 54	56 21	
19					1 46	1	18 56	56 21	
20	1 43	1	18 32	56 22	2 25	1	17 34	56 17	
21	2 15	1	16 52	56 22	3 11	1	16 12	56 50	
22					3 26.	1	15 58	58 6	
23	3 27	1			3 40	1			Iſland Deſcado S.W. ¼ W. 5 lgs. I make Deſcado to to ly in 16 10 N. obs^n. and W. long^de. from London 59 5. The center of the iſland Marigalant N.W. 5 lgs, Dominico S.W. by S. 5 lgs.
29	4. 4	1							In ſight of Antego.
1727 Apr. 14	4 28	1							The high land of the Iſland Nevis W. ½ S^o. 7 lgs. The Iſland RodundaS.by W.¾W. 7 lgs. The center of the Iſland Antego E. by S. ½ S. 7 lgs.

1727

1727 Apr.	Var^n^. P.M.	N^r of Obs.	Lat.	Long^de^.	A.M. Var^n^.	N^r of Obs.	Lat.	Long^de^.	
15	3 30E	2	18 36N	61 18W from London.	2 56E	1	19 20N	61 20W from London.	Lat. of the Island Nevis 17 10 N. Long^de^. W. from London 61 10.
17	3 2	4	19 50	61 14					Bound for England.
19	2 22	3	20 56	61 28					
22	0 51	3	25 20	61 24	1 7	3			
23	0 43	1	25 54	61 23	0 16	2	26 6	61 21	
24					0 15W	2	26 16	61 30	
25					0 41E	1	26 24	61 47	
26					0 18	3	26 34	61 48	Mean of the evening and morning.
27	0 35	1	27 44	61 44					
28					0 38W	4	28 36	60 52	
29	0 42W	2	28 46	60 54	0 48	2	29 6	60 50	
30	1 18	2	30 9	60 45	1 56	1	31 26	60 4	
May 1	2 32	1	32 28	59 33	3 16	1	32 54	59 16	
2	4 5	2	33 54	58 43					
6	9 5	2	40 28	55 42					
7	10 12	1	41 56	52 54					

1727 May	Var^n. P. M.	N^r of Obs.	Lat.	Long^de.	Var^n. A.M.	N^r of Obs.	Lat.	Long^d.	
9	11 54W	2	43 56N	48 41W					This day sounded having 35 fathom fine brown sand; by this we judge we are on the great bank.
10	12 37	3	44 30	46 0					
12	14 20	1	46 12	39 18					A great sea.
13	14 0	1	46 51	35 15					D°. sea.
15	12 54	1	46 16	28 25					D°. sea.
17	11 39	2	46 10	23 53					Smooth water.
21	11 46	2	48 45	15 12					
26	13 24	2							In sight of Portland, dist. about 5 lgs.

Note, By my reckoning I made the Lizard to ly in 50 6 by judgement, and W^t. longitude from London 5 27; and as I have set down the lat. and long^de. at the time of obs^n. by this it may appear how far these observations may be relyed on.

VARIATION of the COMPASS from Madera to the W^{t}. Indies, Lark, Captain GRAY.

1727 xber	Var^{n}. P.M.	N^{r} of Obf.	Lat.	Long^{de}.	Var^{n}. A.M.	N^{r} of Obf.	Lat.	Long^{de}.	
30	6 58W	3							In Madera Road.
Jan.	6 18	1	28 52N	19 46W^{t} from London.					Lat. W^{t}. end of Madera 32 25 N. Long^{de}. W^{t}. from London 17 42.
172$\frac{7}{8}$ 18					2 16E	1	16 19N	51 5W from London.	
19	2 1 E	1	16 29	52 8					
20	2 48	2	16 18	54 53	3 58	1	16 16	56 47	
21	3 58	1	16 24	58 20					
22	3 40	2							Ifland Defcado S.E. $\frac{1}{2}$ E. 8 lgs. N.W^{t}. part of Guardalupa S.W. $\frac{1}{2}$ S. 6 lgs. N.E^{t}. part of Antego N.W. by W. 8 lgs.
23	4 49	2							At the Ifland St. Chriftopher.
27	6 2	2							Ifland Altovalla near Hifpaniola N.E. by N. $\frac{3}{4}$ E. 5 lgs.
Feb. 19	7 37	3							The land called Mother La Pope, near Cartegena, S. by E. 10 or 12 lgs.

VARIATION of the COMPASS in going towards Liſbon from England, and in the Mediterranean, Dreadnought, Capt. GEDDES.

1730 May	Var^n. P.M.	Nr of Obſ.	Lat.	Long^{de}.	Var^n. A.M.	Nr of Obſ.	Lat.	Long^{de}.	
5	13 26W	2	45 22N	9 39W from London.					
7	12 3	2	41 32	10 39					
1731 July 27	12 48	1							Iſland Sizarga S. E. 2 lgs. This iſland is about 5 lgs. to the W'ard of the Groin.
28	12 12	1	41 56	9 26					
29	12 26	1	40 0	10 3					
8ber 8	11 10	1							The land about Cape Dragon in the Mediterranean N. by E. 12 or 14 lgs.
11	10 52	1							Cape Toulon N.W.½W. 6 lgs.
20	9 42	2							In Leghorn Road.
Xber 6	10 48	1	41 34	18 54W from London.					In our paſſage to England.

VARIATION of the COMPASS in the Mediterrannean, Hector, Sir ROGER BULTER.

1733 Apr.	Var^n. P.M.	Nr of Obs.	Lat.	Long^de.	Var^n. A.M.	Nr of Obs.	Lat.	Long^de.	
21	14 2W	1	34 2N	12 30E from London					Between the Island Pantalana and Tripoly in Barbary.
22	13.22	1							The observ^n. not very good. In sight of the land to the E^tard of Tripoly.
26	14 20	2	34 16	12 22					Between Tripoly and the Island Lampodoria.
7ber 17	13 38	3							In Gibralter Bay.
27	13 49	3							Cape St. Vincent N. by E ½ E. 4 lgs
9ber 9	13 56	2							Bound for Gibralter towards Cape de Gatt.
11	14 12	2							Island of Alborne W. by S. 3 leagues.
18	14 34	1							Cape Mola on the Island Minorca N.N.E. 4 or 5 lgs.
Mar. 10	14 20	1							Cape St. Marys on the Coast of Portugal N.E. by E. 6 or 7 lgs.
1734 Apr. 6	13 56	3							On the Coast of Barbary near Larac.

1735 Aug.	Var^n. P.M.	Nr of Obs.	Lat.	Long^de.	Var^n. A.M.	Nr of Obs.	Lat.	Long^de.	
9	11 33W	2	38 8N	14 2W from London.					Going from the Coast of Barbary to Lisbon.
10	11 44	1	38 8	12 55					
1735 Aug.	12 19	3							In Sal'ee Road on the Coast of Barbary.

In Scandaroon Bay, 1711, I found 16^d W. variation.

At Elsinore and Carlescroon in the Baltic, 1716, I found a a point W^t. variation.

III. *Pro-*

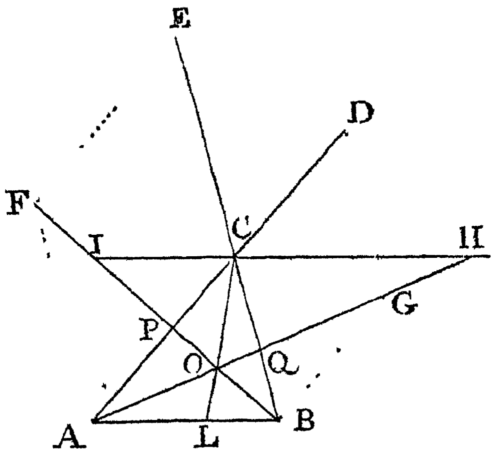
III. *Propositions selected from a Paper on the Division of Right Lines, Surfaces, and Solids. By* James Glenie, *A. M. of the University of* Edinburgh. *Communicated by the* Astronomer Royal.

R. June, 1775.

PROPOSITION I. THEOREM.

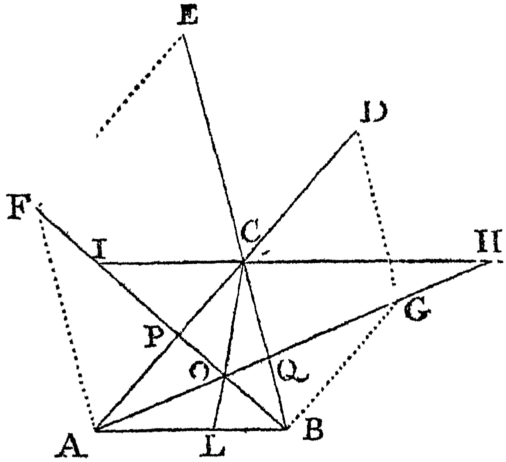
If from the angles at the base of any right-lined triangle, right lines be drawn to the alternate angles of rhombi, *described upon the opposite sides, and applied reciprocally to the sides produced; and from the vertex, through the intersection of these lines, a right line be drawn to meet the base: the segments of the base, made thereby, will have to each other the duplicate proportion of the sides.*

LET ACB be any right lined triangle. Let AFEC, CDGB be rhombi, on any two sides AC, CB of this triangle, applied respectively to CB, AC, produced: from the alternate angles EFA, DGB, of which let FA, GA, be right lines drawn to the angles at the base, or



third side, AB. Then, if through the intersection O of

these



these lines, a right line COL be drawn from the vertex C to meet the base AB; the segments AL, LB, of the base made thereby, will have to each other the duplicate proportion of the sides AC, CB. For through the vertex C let there be a right line drawn parallel to AB, to meet BF, AG produced, if necessary. Then, since the triangles CQH, CPI, are respectively equiangular to the triangles AQB, APB (15. and 29. E. 1.); the proportions of CH to AB and of AB to IC are respectively equal to the proportions of CQ to QB and of AP to PC (4. E. 6.). But the proportion of CH to IC is compounded of the proportion of CH to AB, and of AB to IC; and consequently is equal to the proportion compounded of the proportions of CQ to QB, and of AP to PC. And, since the triangles ACQ, APF, are respectively equiangular to the triangles BQG, BPC (15. and 29. E. 1.); the proportions of CQ to QB and of AP to PC are each equal to the proportion of AC to CB (4. E. 6.); and when compounded are equal to the duplicate proportion of AC to CB. Wherefore the proportion of CH to IC, which hath been shewn to be equal to the proportion compounded of the proportions of CQ to QB and of AP to PC, is also equal to the duplicate propor-

tion of AC to CB (11. E. 5.). But, ſince the triangles COH, COI, are reſpectively equiangular to the triangles AOL, LOB, the proportion of CH to IC is equal to the proportion of AL to LB (4. E. 6. and 16. E. 5.). Therefore the proportion of AL to LB is equal to the duplicate proportion of AC to CB (11. E. 5.). *Q. E. D.*

COR. I. If the triangle be iſoſceles, the right line drawn from the vertex to the baſe is perpendicular thereto, and the ſegments of the baſe are equal to each other.

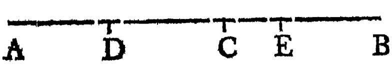
COR. II. When the triangle is right-angled, the line drawn from the vertex to the baſe is always perpendicular to it (as appears from 8. E. 6. and its cor.); and the *rhombi* become ſquares on the ſides comprehending the right angle.

COR. III. The ſegments of the ſides adjacent to the baſe, are reſpectively third proportionals to the ſum of the ſides, and the ſides themſelves.

COR. IV. The ſegments of the ſides adjacent to the vertex are equal to each other, and each of them is a fourth proportional to the ſum of the ſides, and the ſides themſelves (a).

COR.

(a) And it may be added, a mean in proportion between the two ſegments adjacent to the baſe. For if a right line AB be any how divided in C, and from the two ſegments CA, CB, third proportionals to the whole line and each ſegment



reſpectively, CD, CE, be taken away, the remainders AD, EB, are equal, and each is a mean in proportion between the two CD, CE. For becauſe AB : AC = AC : CD; therefore,

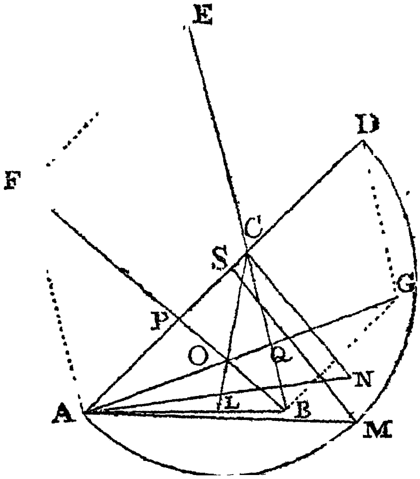
COR. V. The segments of the base are proportional to the segments of the sides, which are adjacent to them.

PROPOSITION II.

Let there be any two right lines given. There is an angle which may be made by these lines; such that if, from their extremities which do not meet, right lines be drawn to the alternate angles of rhombi described on them, and reciprocally applied to them when produced; and from the said angle through the intersection of these lines, a right line be drawn to meet the right line joining the said extremities; the segments of this line made thereby, shall be respectively equal to the adjacent segments of the given lines.

therefore, by conversion, AB : BC = CA : AD. Again, because AB : BC = BC : CE, by conversion AB : AC = BC : BE: and by permutation AB : BC = AC : BE. Therefore AC : BE = AC : AD. Therefore AD and BE are equal. I say, moreover, that each of the two equal lines AD, BE, is a mean in proportion between the two CD, CE. For because BA : AC = AC : CD, by division BC : CA = AD : DC. Again, because BA : BC = BC : CE, converting and dividing BC : CA = CE : EB. Therefore, CE : EB = AD : DC. *Q. E. D.* S. HORSLEY.

LET

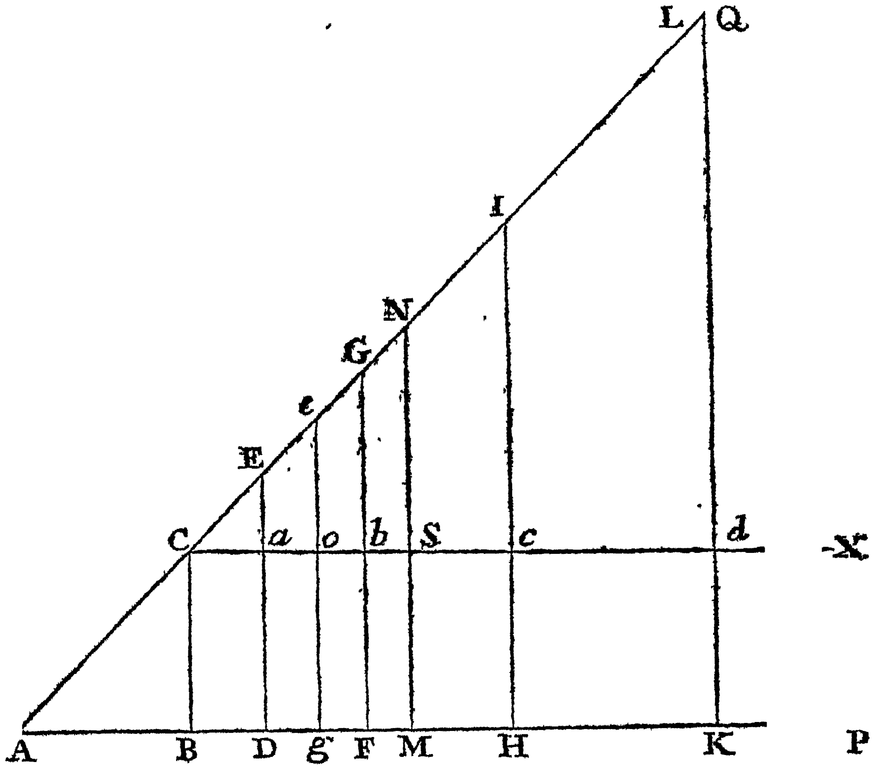


LET AC, CB, be any two given right lines, and let CD in AC produced be equal to CB. On AD defcribe a femi-circle; draw CN at right angles to AD, and equal to CD (11. E. 1.); join A, N, and apply a right line AM in the femi-circle equal to AN (1. E. 4.). From the point M draw the right line MS at right angles to AD (1. E. 4.) Make a triangle ACB, having its fides equal to AC, AS, and CB (by 22. E. 1.); and ACB is the angle required to be found; and the fegments AL, LB, of the right line AB joining the extremities A and B of the given lines are refpectively equal to the fegments AP, BQ, of the given lines, which are adjacent to them. For the fquare on BC hath to the fquare on AC the duplicate proportion of BC to AC (cor. 1. to 20. E. 6.); that is, the proportion of BL to LA (prop. 1.). Wherefore the fquares on AC, CB; that is, the fquare on AN or AM (47. E. 1.) hath to the fquare on AC the proportion of AB to AL (18. E. 5.). But the fquare on AN or AM is equal to the rectangle contained by AD, AS (8. and 17. E. 6.). Wherefore the rectangle contained by AD, AS, hath to the fquare on AC, the proportion of AB to AL; that is, the proportion of the rectangle contained by AB, AS to the rectangle contained by AL, AS (1. E. 6.). Confequently, the proportion of the rectangle AD, AS, to the rectangle

rectangle AB, AS; that is, the proportion of AD to AB (1. E. 6.) is equal to the proportion of the square on AC to the rectangle AL, AS (16. E. 5.) But AD hath to AC the proportion of AC to AP (cor. 3. to prop. 1.). Therefore, the rectangle AD, AC, hath to the square on AC the proportion of the rectangle AC, AS, to the rectangle AP, AS (1. E. 6.). Therefore AD hath to AS or AB the proportion of the square on AC to the rectangle AP, AS (16. E. 5. and 1. E. 6.). Hence the rectangle AL, AS, is equal to the rectangle AP, AS (11. and 16. E. 5.), and AL, AP, are consequently equal. But as AL to AP, so is BL to BQ (by cor. 5. prop. 1.). Therefore BL, BQ, are likewise equal. *Q. E. D.*

PROPOSITION III. PROBLEM I.

To multiply the square of a given finite right line by any number.



UPON an indefinite right line AP set off the given right line AB; draw BC at right angles to AP and equal to AB; and from A through C draw an indefinite right line AQ. Take AD equal to AC, and draw DE parallel to BC; AF equal to AE; and draw FG parallel to BC, and so on. Then it appears (from 47. E. 1.), that the square of AC is

is equal to the ſquare of AB multiplied by 2; the ſquare of AE equal to the ſquare of AD or AC multiplied by 2; that is, equal to the ſquare of AB multiplied by 4, and ſo on. Thus the ſquares of AC, AE, AG, AI, AL, &c. are reſpectively equal to the ſquare of AB multiplied by the terms of the following ſeries 2, 4, 8, 16, 32, 64, &c. where the ſixty-third term gives the ſquare of AB multiplied by the laſt term of SESSA's Series for the Cheſs-board.

If CX be drawn parallel to AP, the ſquares of A*a*, A*b*, A*c*, A*d*, &c. will be reſpectively equal to the ſquare of AB multiplied by 3, 5, 9, 17, 33, 65, 129, &c. Alſo if A*g* be taken equal to A*a*, and *ge* be drawn parallel to BC, and this be repeated, the ſquares of A*e*, &c. will be equal reſpectively to the ſquare of AB multiplied by 6, 12, 24, 48, &c. And the ſquares on A*o*, &c. will be equal to ſquare on AB multiplied by 4, 7, 13, 25, 49, &c. In like manner, if AM be taken equal to A*b*, and MN be drawn parallel to BC, the ſquares on AN, &c. will be equal reſpectively to the ſquare on AB multiplied by 10, 20, 40, 80, 160, &c. And the ſquares on AS, &c. will be equal reſpectively to the ſquare on AB multiplied by the terms of the following ſeries: 6, 11, 21, 41, 81, 161, &c.

In the ſame way, if right lines be drawn from E, *e*, G, N, I, L, &c. there will ariſe numberleſs other ſeries. And if BC be taken equal to AB multiplied by any number, ſurd, fractional, or mixed, there will be obtained a great variety of ſeries, conſiſting reſpectively of terms, which are

are ſurd, fractional, or mixed. And by dividing BC, DE, *ge*, FG, MN, HI, KL, in different ways, according to pleaſure, we may apply the ſame method to fractional numbers, without altering the magnitude of BC. Thus, if BC be biſected, and a right line be drawn through the point of biſection parallel to AP, there will be found right lines, the ſquares on which are reſpectively equal to the ſquare on AB multiplied by a great number of fractions, having four for their common denominator, and ſo on.

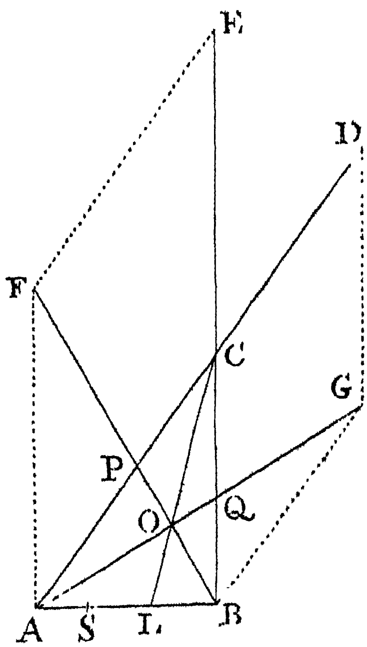
PROPOSITION IV. PROBLEM II.

To find a right line, the ſquare on which ſhall be equal to the ſquare on a given right line, divided by any number.

IF, uſing the figure of the immediately preceding problem, we ſuppoſe the given right line to be denoted by AL, the ſquares on AK, AH, AF, AD, AB, &c. will reſpectively be equal to the ſquare on AL multiplied by $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{32}$, $\frac{1}{64}$, $\frac{1}{128}$, $\frac{1}{256}$, $\frac{1}{512}$, $\frac{1}{1024}$, &c. or divided by 2, 4, 8, 16, 32, 64, 128, 256, 512, 1024, &c.; and ſo on for other numbers, whole, ſurd, fractional, or mixed.

PROPOSITION V. PROBLEM III.

To cut off from a given right line a part expressed by any odd number.



LET AB be the given right line. At right angles to it, at one of its extremities B, draw an indefinite right line BE. Let n be the given odd number, which expresses the part of AB which is to be cut off. Take BC such a right line (prob. 8.), that the square on it shall be equal to the square on AB, multiplied by the number $\frac{n-1}{2}$. Draw CL as in the first theorem, and take LS equal to LB. I say, AS is that part of AB, which is expressed by the odd number n.

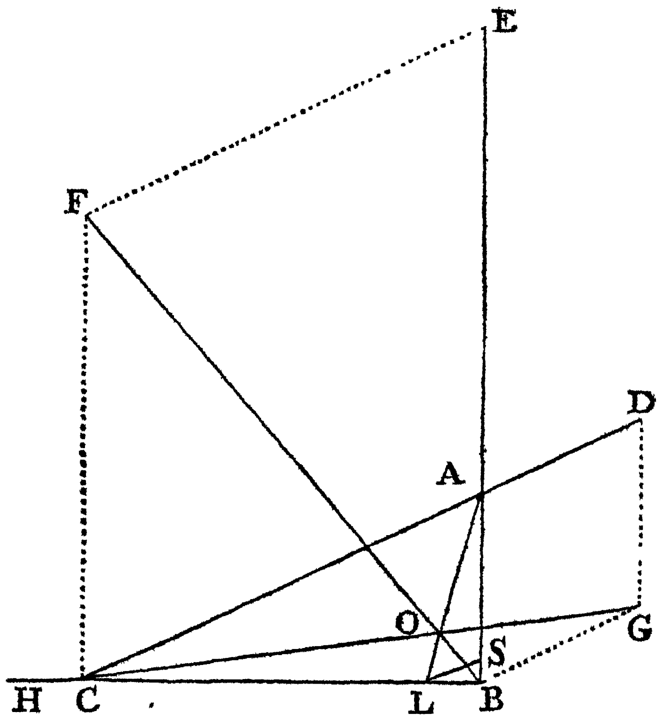
For the square on AC being equal to the squares on AB, BC, is equal to the square on AB multiplied by the number $\frac{n-1}{2}+1$, or $\frac{n+1}{2}$. Therefore it appears (from prop. 1. and cor. 1. to 20. E. 6.), that AL is to LB as $\frac{n-1}{2}+1$ to $\frac{n-1}{2}$. Consequently, AS is equal to the part required. *Q. E. F.*

3 Thus,

Thus, if the ſquare on BC be ſuppoſed ſucceſſively equal to the ſquare on AB multiplied by the terms of the ſeries 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, &c. the numbers of the ſeveral parts denoted by AS, will be 11, 13, 15, 17, 19, 21, 23, 25, 27, 29, 31, 33, 35, 37, 39, 41, &c. which ſeries comprehends all odd numbers after 9, and might have begun from 3 had the other ſeries begun from 1.

PROPOSITION VI. PROBLEM IV.

To cut off from a given right line a part expressed by any even number.



Let *m* denote any even number in general. Draw any indefinite right line BH, and at right angles to it another BE. On BE set off the given right line BA, and from A, with the distance equal to a right line, the square on which is equal to the square on AB multiplied by the number

number $m-1$, interſect BH in ſome point C. From the vertex A of the triangle BAC draw AL as was directed in prop. 1. and draw LS parallel to CA. I ſay BL is ſuch a part of BC as is expreſſed by the number m; and that BS is the ſame part of AB. For it appears (from prop. 1.) that BL is to CL as 1 to $m-1$. Wherefore BL is the m^{th} part of BC. And BS is the ſame part of AB that BL is of BC (4. E. 6.)

Thus if the ſquare on AC be ſucceſſively denoted by the ſquare on AB multiplied by 3, 5, 7, 9, 11, 13, 15, 17, 19, 21, 23, 25, &c. BS will be ſucceſſively ſuch a part of AB as is expreſſed by 4, 6, 8, 10, 12, 14, 16, 18, 20, 22, 24, 26, 28, &c.

PROPOSITION VII. THEOREM II.

If from the angles at the baſe of any right lined triangle, right lines be drawn to the alternate angles of rhombi *deſcribed on the other two ſides, and reciprocally applied to them produced, and through the interſection of theſe lines, a right line be drawn from the vertex to the baſe; the rectangle contained by the ſines of the angles at the extremities of one of the ſides, will be equal to the rectangle contained by the ſines of the angles at the extremities of the other* (b)*; and the parallelepiped contained by the ſines of the*

(b) The author means, that $\overline{\text{ſin. ACL} \times \text{ſin. CAL}} = \text{ſin. BCL} \times \text{ſin. CBL}$ (ſee fig. prop. 1.). For ſin. ACL : ſin. L = AL : AC, and ſin. L : ſin. BCL = BC : BL. Take N, a third in proportion to AL, AC. Then, becauſe $AC^2 : BC^2 = AL : LB$, N will likewiſe

the angles of one of thofe triangles, into which the original one is divided by the faid line drawn from the vertex. will be equal to the parallelepiped contained by the fines of the angles of the other.

COR. The two triangles, adjacent to the fegments of the bafe, have to each other the proportion of the two adjacent to the fides containing the vertical angle, or the proportion of the two into which the original triangle is divided; and any one of thefe pairs of triangles are as fimilar figures defcribed on the fides, being as the fegments of the bafe, which have to each other the duplicate proportion of the fides.

PROPOSITION VIII. THEOREM III.

If from the angles at the hypotenufe of any right angled right lined triangle, right lines be drawn to the alternate angles of fquares defcribed on the fides containing the right angle, and from the point where the right line drawn from the right angle, through their interfection, meets the hypotenufe, right lines be drawn to the points, where thefe lines meet the fides; the lines fo drawn will

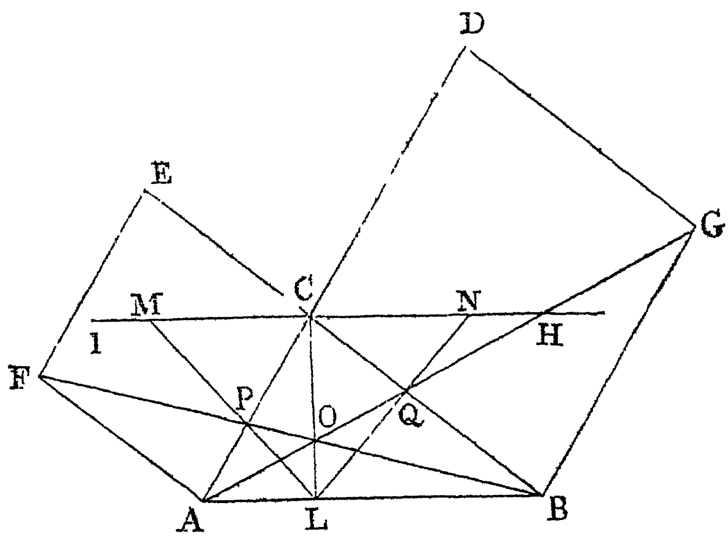
likewife be a third in proportion to LB, BC. Hence, fin. ACL : fin. L = AC : N, and fin. L : fin. BCL = N : BC. *Ex æquo perturbate* fin. ACL : fin. BCL = AC : BC. But fin. CBL : fin. CAL = AC : BC. Therefore, fin. ACL : fin. BCL = CBL : fin. CAL, and $\overline{\text{fin. ACL} \times \text{fin. CAL}} = \overline{\text{fin. BCL} \times \text{fin. CBL}}$. *Q. E. D.* And hence, fin. L × fin. ACL × fin. CAL = fin. L × fin. BCL × fin. CBL, which is the fecond branch of the propofition. S. HORSLEY.

make

make equal angles with the hypotenuse, and the right line drawn from the right angle to meet it; and will likewise have to each other the proportion of the sides containing the right angle (c).

COR.

(c) For in the triangle ACB, having the angle at C right, construct the figure as in the first proposition, and join LP, LQ. The author affirms, that the angles



PLA, QLB, and also the angles CLP, QLC, are equal, and that PL : LQ = CA : CB. Produce LP, LQ, till they meet the right line IH in M, N. Now, because MN, AB, are parallel, therefore AL : MC = AP : PC, and CN : LB = CQ (or PC) : QB. Therefore, AL × CN : MC × LB = AP : QB. But AP : QB = AL : LB (by theor. 1. cor. 5.). Therefore, AL × CN : MC × LB = AL : LB. Therefore, CN and CM are equal. But because the angle ACB is right, CL is perpendicular to AB (by theor. 1. cor. 2.), and consequently to MN. Therefore the angles MCL, NCL, are right, and MC being equal to CN, and CL common to the two triangles MCL, NCL, ML will be equal to LN, and the angles MLC, CML, equal to NLC, CNL, respectively. Hence it is evident, that the angles CLP, CLQ, are equal, and each is half a right angle; and likewise PLA, QLB, are equal, and each half a right angle.

Further,

COR. I. The alternate triangles of thoſe four, which have their vertices in the point, where the right line drawn from the right angle meets the hypotenuſe, are ſimilar, and have to each other the proportion of the ſegments of the hypotenuſe, or the duplicate proportion of the ſides containing the right angle.

COR. II. Either pair of the adjacent triangles lying on different ſides of the right line drawn from the right angle, and having their vertices in the interſection of the right lines drawn from the angles at the hypotenuſe, have to each other the proportion of the alternate triangles, having their vertices in the interſection of the firſt-mentioned line and the hypotenuſe.

COR. III. The trapezium or quadrilateral figure formed by the ſegments of the ſides adjacent to the right angle, and the right lines joining their extremities with the interſection of the hypotenuſe, and the right line drawn from the right angle to meet it, is capable of being inſcribed in a circle; and is divided at the interſection of right lines drawn from the angles at the hypotenuſe to the alternate angles of ſquares, deſcribed on the ſides containing the right angle, into triangles which are proportional to one another, and when taken two by two, as

Further, the angles, PLA, CLQ, are equal, each being half a right angle. And the angles PAL, LCQ, are equal (by 8. Elem. 6.). Therefore the angles APL, CQL, will be equal, and the triangles APL, CQL, ſimilar, and the ſides ſubtending the equal angles proportional. Therefore, PL : LA = QL : LC. By permutation, LP : LQ = LA : LC. But LA : LC = AC : CB (by 8. Elem. 6.). Therefore, LP : LQ = AC : CB. *Q. E. D.* S. HORSLEY.

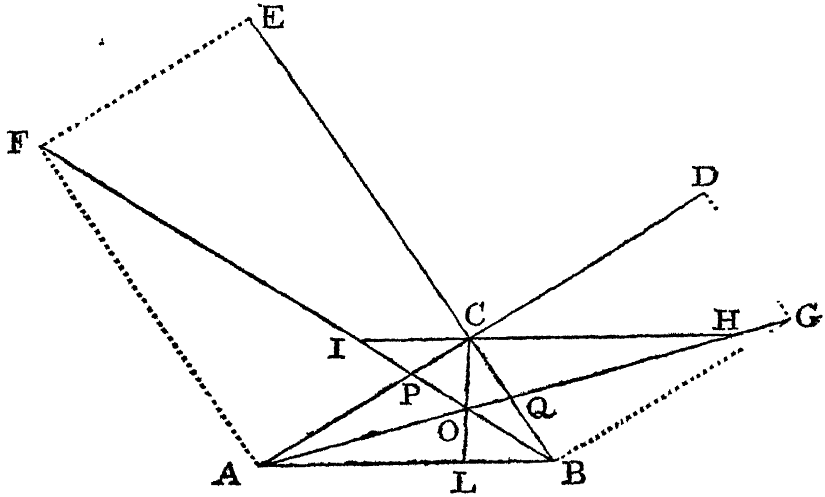
they

they lie adjacent on different fides of the diagonal, are proportional to the unequal fides of the trapezium, and to the two triangles into which the diagonal divides it. For OPL : OPC=LO : OC=OQL : OQC. Therefore, OPL : OQL =OPC : OQC=LPC : LQC=LP : LQ.

PROPOSITION IX. THEOREM IV.

If from the angles at the bafe of any right lined triangle, right lines be drawn to the alternate angles of rhomboids defcribed on the other two fides, and reciprocally applied to them produced, a right line drawn from the vertex through the interfection of thefe lines will cut the bafe into two parts, having to each other the proportion compounded of the proportion of the fides, and of the proportion of the other two lines comprehending the rhomboids.

I SHALL fubjoin a demonftration of this theorem, fince the firft propofition in thefe papers is only a particular cafe of it.



Let the triangle be ACB, the base AB, the rhomboids ACEF, CCDG; and let the right lines BF, AG, be drawn. Then, if from the vertex C through their intersection O, a right line COL be drawn to meet the base, the segments AL, LB, thereof will have to each other the proportion compounded of the proportions of AC to CB, and of CE to CD. For through the vertex C let a right line ICH be drawn parallel to AB, to meet BF, AG, produced, if necessary. Then, since the triangles CQH, CPI, are respectively equiangular to the triangles AQB, APB (15. and 29. E. 1.); the proportions of CH to AB, and of AB to IC are respectively equal to the proportions of CQ to QB, and of AP to PC (4. E. 6.). But the proportion of CH to IC is compounded of the proportions of CH to AB and of AB to IC; and consequently is equal to a proportion compounded of the proportions of CQ to QB and of AP to PC. And since the triangles ACQ, APF, are respectively equiangular to

 the

the triangles BQG, BPC (15. and 29. E. 1.); the proportions of CQ to QB, and of AP to PC, are respectively equal to the proportions of AC to BG or CD, and of AE or CE to CB. Wherefore the proportion of CH to IC is equal to the proportion compounded of the proportions of AC to CD and of CE to CB, or of AC to CB and of CE to CD. But since the triangles COH, COI, are respectively equiangular to the triangles AOL, LOB, the proportion of CH to IC is equal to the proportion of AL to LB (4. E. 6.). Therefore the proportion of AL to LB is equal to the proportion compounded of the proportions of AC to CB and of CE to CD. *Q. E. D.*

SCHOLIUM. If CE, CD, be equal to each other, AL hath to LB the proportion of AC to CB, and CL bisects the angle ACB; if CE have to CD the inverse proportion of AC to CB, AL is equal to LB; if CE have to CD the proportion of AC to CB, AL hath to LB the duplicate proportion of AC to CB; and universally, if CE have to CD any multiplicate proportion, n, of AC to CB, AL hath to LB such a multiplicate proportion of AC to CB as is expressed by the number $n+1$. And if CE have to CD any multiplicate proportion m of CB to AC, AL will have to LB such a multiplicate proportion of CB to AC, as is expressed by the number $m-1$.

IV. *A new Method of finding Time by equal Altitudes.* *By* Alexander Aubert, *Esq. F. R. S.*

R. Nov. 9, 1775.

AMONG the various methods practised for finding time, that by equal altitudes of the Sun or of a star, hath hitherto been esteemed the most eligible for observers who are not furnished with a good and well-adjusted transit instrument. But this method, although one of the best, is generally attended with inconveniencies, which render the practice of it more difficult, and the result less perfect than one could wish.

If the Sun or stars near the equator are made use of, as usual, and the altitudes are taken near the prime vertical, where the change of altitude is the quickest, the interval of time between the observations must, in most latitudes, be of so many hours, that the observer cannot always attend to the corresponding altitudes: the weather may prove variable, so as to disappoint him at last; the clock or watch may go irregularly during so long an interval; and if the altitudes cannot, on account of their great distance from the meridian, be taken very high; an alteration in the state of the atmosphere may produce a variation of the refraction, and occasion the horary arcs to be different, although the apparent altitudes will be the same. To which may be added, the difficulty of making

making the inſtrument follow the object in its motion in azimuth, without danger of diſturbing its adjuſtment in regard to altitude.

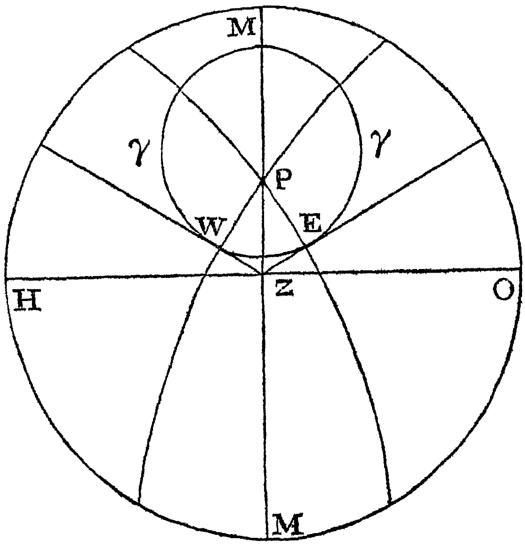
To remedy all theſe inconveniencies, the following method was thought of; and having been practiſed with conſtant ſucceſs, it is preſumed, the communication of it may be acceptable to aſtronomers.

If a ſtar is ſelected, of which the polar diſtance is very little leſs than the complement of the latitude of the place of obſervation, it will, at equal diſtances from the meridian, come to vertical circles, which touch its parallel of declination. The ſtar, when in theſe vertical circles, will be near the meridian, near the prime vertical, and near the zenith; and conſequently, if it be obſerved there, the interval between the Eaſtern and the Weſtern altitudes will be ſhort; the alteration in altitude will be quick; the ſtar cannot be affected by a different refraction; beſides, it will have no motion in azimuth.

To obſerve the ſtar in theſe vertical circles, two things are neceſſary; the firſt is, to be provided with an aſtronomical quadrant, having three or more horizontal wires in the teleſcope, and if it have alſo a *ſpeculum* at the eye-end of the teleſcope, to bring the vertical ray horizontal, it will be found very convenient. The next thing is, to make a computation of the apparent zenith diſtance of the ſtar in the vertical circles which touch its parallel of declination; for if the teleſcope be fixed to this zenith diſtance, as ſoon as the ſtar is found

found to come to it, it will be in the proper vertical circle.

The true zenith diſtance is found eaſily in the following manner; and if diminiſhed by the refraction belonging to it, it will give the apparent zenith diſtance wanted.



Let HMOM repreſent the horizon; Z the zenith; MM the meridian; P the pole of the equator; γγ the parallel of declination of a ſtar, interſecting the meridian very near the zenith between Z and P. Then, the vertical circles ZE on the Eaſtern ſide of the meridian, and ZW on the Weſtern ſide, being drawn to touch the path of the ſtar, the hour circles paſſing through the points of contact E, W, will be at right angles to the circles of azimuth ZE, ZW.

Now in the right-angled ſpheric triangle PEZ; PZ the complement of the latitude, and PE the ſtar's polar diſtance, being known, the other parts are eaſily found; *viz.* ZE the zenith diſtance, PZE the azimuth, ZPE the horary angle. So that the teleſcope may be fixed properly, the poſition of the inſtrument is aſcertained, and the time known when the ſtar may be expected in the field

field of the teleſcope; and if the vertical wire is once brought near to the ſtar, the ſtar will appear to move parallel to it, and will paſs ſucceſſively over the horizontal wires, while the inſtrument remains undiſturbed.

The advantage of this method will appear in the following example of equal altitudes, taken the 15th July, 1773, at Loam-pit Hill, near Deptford, in latitude 51° 28′ 7″ N. and longitude 5″ in time W. of the Royal Obſervatory at Greenwich.

The ſtar ſelected was γ Draconis, having 38° 28′ 21″ apparent North polar diſtance, being very little leſs than the complement of the latitude 38° 31′ 53″.

Then coſ. PE : rad. :: coſ. PZ : coſ. ZE = 2° 19′ the zenith diſtance,
and ſin. PZ : rad. :: ſin. PE : ſin. PZE = 87° 5′ 20″ the azimuth,
alſo rad. : tan. PE :: cotan. PZ : coſ. ZPE = 3° 43′ 13″ the horary arc = 14′ 52″,9
in ſidereal time, or 14′ 50″,5 in mean time.

The true zenith diſtance being 2° 19′, the ſame was diminiſhed by 2″ for refraction, and the teleſcope fixed to 2° 18′ 58″, the apparent zenith diſtance; and when the ſtar came to the wires, the times by the clock were as follow:

	Eaſtern altitudes. h ′ ″			Weſtern altitudes. h ′ ″	Meridian paſſage. h ′ ″
1ſt wire at	9 55 43	2′ 14″	2′ 14″	10 29 46	10 12 44,5
2d	9 57 57	2 12	2 12	10 27 32	10 12 44,5
3d	10 0 9			10 25 20	10 12 44,5

ſo that in about 34′ the compleat ſet of altitudes was obtained near the prime vertical, free from the effects of a different refraction, and any motion in azimuth. The horary

horary arc obſerved by the middle wire not turning out exactly according to the computation, is of no conſequence to the obſervations. Some little difference may ariſe in it from ſmall inaccuracies in the eſtimation of the ſtar's apparent polar diſtance, the latitude of the place, or the error of the line of collimation; or from not ſetting the teleſcope exactly to the proper zenith diſtance; but as the chief intention of the computation is to find the vertical circles in which the ſtar hath no motion in azimuth, the other parts of it need not be ſtrictly attended to.

The following manner of inferring mean time from the ſtar's meridian paſſage being more convenient and conciſe than the uſual one, may alſo be acceptable.

From the ſtar's apparent right aſcenſion, increaſed by 24 hours if neceſſary, ſubtract the Sun's apparent right aſcenſion for apparent noon; diminiſh the remainder by the proportional part of the ſtar's acceleration, at the rate of 3′ 55″,91 for 24 hours, of which a table is eaſily computed; to this laſt remainder apply the equation of time for apparent noon, according as it is additive or ſubtractive; the reſult will be the mean time of the ſtar's paſſing the meridian.

EXAMPLE.

EXAMPLE.

	h	′	″
The apparent Æ of γ Draconis the 15th July, 1773, was	17	51	24,0
—the apparent Æ of the Sun at apparent noon,	7	39	59,0
First remainder,	10	11	25,0
— the ſtar's acceleration for 10ʰ 11′ 25″, at 3′ 55″,91 for 24 hours		1	40,2
Second Remainder,	10	9	44,8
+ the equation of time at apparent noon, additive,		5	27,7
Star's meridian paſſage in mean time,	10	15	12,5

But the clock ſhewed 10ʰ 12′ 44″,5 when the ſtar paſſed, conſequently it was 2′ 28″,0 too ſlow for mean time.

Obſervers, who are not furniſhed with tables of the Sun's right aſcenſion and of the equation of time for the apparent noon of their meridian, may apply both as they are given in the Nautical Ephemeris for the meridian of the Royal Obſervatory at Greenwich; the reſult will be the mean time of the ſtar's paſſing the Greenwich meridian. And by applying the proportional part of the foregoing acceleration of 3′ 55″,91, belonging to the difference of longitude in time of the place of obſervation from Greenwich, the mean time of the ſtar's paſſing the meridian of the place of obſervation will be found. If the place be to the Eaſt of Greenwich, the acceleration will be additive; if to the Weſt, ſubtractive.

In a ſimilar manner, the mean time of any obſervation made with a clock, regulated to ſidereal time, may be inferred, provided the preceding tranſit of the Sun hath been obſerved; for if from the time of the obſervation by the clock, increaſed, if neceſſary, by 24 hours, the time of the obſerved tranſit of the Sun be ſubtracted, the remainder, diminiſhed by the proportional part of 3′ 55″,91 and duly corrected by the equation of time for the preceding noon, will give the mean time required. It is underſtood, that the clock keeps the rate of ſidereal time exactly; for if not, a further correction for the loſs or gain ſince noon muſt be applied.

V. *An*

V. *An Account of* Falkland Islands. *By* William Clayton, *Esq. of his Majesty's Navy.*

R. Nov. 9, 1775.

FALKLAND's Islands, or, as the Spaniards and French call them, the Maloine Islands, are situated between the latitude of 52° 26′ and 51° 6′ S. and longitude from London 56° to 60° 30′ W. They are numberless, forming a mass of broken high lands, or very low sedgy keys and sunken rocks. The largest is the Easternmost island, and on the Eastern side the Spaniards had a settlement, which the crown of Spain purchased of M. BOUGAINVILLE, who, on his private account, had formed a settlement in the year 1764, at the time that Commodore BYRON had first discovered Port Egmont. The next large island is of a very considerable extent, and hath many excellent harbours on it. Between these two runs Falkland's Sound, which is navigable through; but the South entrance is pretty full of low sandy keys. Adjoining to the second large island, to the Westward, lies Saunders's Island, on which the English settlement was made, a blockhouse erected, several spots inclosed for gardens and three storehouses, and five dwelling-houses or huts, built at different times by the ships crews who were stationed there. The harbour of Port Egmont was formed by these islands, and another

another high, barren, rocky iſland, named Kepple's Iſland, and ſome other leſſer iſlands to the N.E. and Eaſtward, and was intirely land-locked, or incloſed by the land, on every point: it was very ſpacious; the bottom was muddy and good holding ground. From the hills through the bogs drained ſeveral runs of water, and as the landing-places were good, and a natural ſmall cove for boats to lye in ſafety on the North-ſide of Saunders' Iſland ſheltered from the S.W. winds, it induced Captain MACBRIDE to begin the ſettlement on it.

The larger iſlands are overſpread with a ſhort, tufty, round graſs; a ſhrub with a ſmell like roſemary; a ſhrub of the myrtle kind, which in March and April bloſſoms (a); a white flower, of a faint violet ſmell; a ſmall annual plant, of the wormwood kind. Near the ſhore, where-ever there is a ſandy ſoil, a ſpecies of graſs grows, called Penguin graſs, from the birds of that ſpecies making their neſts, and burrowing under ground like rabbits in holes. This graſs grows four or five feet high; the blades are broad and coarſe like ruſhes; the roots, when roaſted, eat like almonds. Ground-ſorrel every where abounds in the greateſt plenty; is extremely tart, and a moſt excellent antiſcorbutic; the flower it produces is exactly like the wild roſe which grows in the hedges in England. Celery, pepper-graſs, and ſcurvy-graſs, alſo abounds upon every iſland. Maidenhair (improperly ſo called) is plenty; the berries are ripe in February and March, and very plea-

(a) Lieutenant CLAYTON hath two or three of theſe ſhrubs in his garden at Peckham.

ſant,

ſant. A ſmall ſpecies of cranberry abounds, and is the food of the wild geeſe all the autumn, when the geeſe are beſt. In the ſpring ſeaſon, and part of the ſummer, there ſprings up an extreme pretty humble flower, which neareſt reſembles in leaf the auricula, but in flower the primroſe; only they blow quite white. In very barren craggy ſpots, and even out of the clifts of the rocks near the ſea-ſhore, grows in the ſummer ſeaſon, a ſmall ſhrub which produces an uncommon but pretty flower, ſhaped like a lady's pocket; the colour is a rich yellow, I termed it queen's pocket: the ſeeds are very ſmall. Theſe are all the natural vegetable productions, and nothing riſes to any ſize, nor doth any tree grow, on any of theſe iſlands (b).

The prevailing winds are from the S. to the W. for two-thirds of the year, and in general boiſterous and ſtormy. The N. and N.W. winds are mild and warm; but ſeldom of long continuance. The winds from the N.E. are moiſt, foggy, and unwholeſome. From E. to S. are moſt pernicious, blighting, and tempeſtuous; they affect man, bird, beaſt, and vegetation: nothing can ſtand it which is expoſed. Happily its duration is ſhort; it ſeldom continues above 24 hours. It cuts the her-bage down as if fires had been made under them; the leaves are parched up, and crumble to duſt. The fowls are ſeized with cramps, ſo as to become lame, and never

(b) Wood ſtrawberries grow on theſe iſlands, and are ripe in March; are of an earthy inſipid taſte, and grow to the ſize of the common ſmall ſtrawberry in England.

recover;

recover; but continue to decline till the whole ſide is decayed which was firſt affected. Hogs and pigs are ſuddenly taken with the ſtaggers, turn round and drop, never to recover. Men are oppreſſed with a ſtopped perſpiration, heavineſs at the breaſt, ſore throats; but they ſoon get over it, by due care.

The ſea abounds with mullets, and ſome of a very large ſize up to ten pounds weight. Smelts in abundance, and as large as fourteen and fifteen inches in length; I have taken ſuch with an angling line and rod. Tranſparent fiſh, ſhaped like a pike about the head, but not larger than a herring: theſe tranſparent fiſh are ſo clear when caught, that you may ſee through them; they have no red blood, but when cut a ſlimy water iſſues out, which I ſuppoſe is their blood. There are three or four ſpecies of the common loggerhead, or *ſculpa* fiſh, common on the Engliſh coaſts. A ſmall ſand-crab, ſmall cray-fiſh, are to be got. Muſcles are plenty, with limpets, and a few ſmall clams. The muſcles are very large and fine, and no way dangerous. In the river on the large iſland, are ſmall fiſh like trout, very delicious; and no other ſort whatever.

The amphibious animals are of four kinds, though ſeemingly of the ſame *genus*: the ſea-lyon and the ſeal are diſtinct; the clapmatch ſeal and the fur ſeal are alſo diſtinct animals. The ſea-lyon and lyoneſs are bull-faced, with long ſhaggy hair; the common ſeal is ſmooth; the clapmatch is beſt pictured in Lord ANSON's voyage, under the name of ſea-lyon, in the drawings; the furr

ſeal

feal has its name from its coat, which is a fine foft furr, and is thinner fkinned than any of the others. They all come on fhore in December, to whelp their young; and remain moftly on land till they engender again. During this feafon it is rather dangerous coming near them, for the males are then vicious, and will endeavour to hurt any one who approaches their females; but at all other times they endeavour to make to the water, where they are fafe. In mild warm days, during the fummer, they come on fhore, and lye bafking in the Sun.

I confider the penguins as amphibious animals, partaking of the nature of birds, beafts, and fifhes. There are four kinds; the yellow, or king penguin; the red; the black or holey, from their burrowing under ground; and the jumping jacks, from their motion. Thefe creatures generally live in the fea, have very fhort wings which ferve for fins, are covered with fhort thick feathers, and fwim at an amazing rate. On fhore they walk quite erect with a waddling motion, like a rickety child; and their breafts and bodies before being quite white, at a diftance have, at firft fight, the look of a child waddling along with a bib and apron on. They come on fhore to lay and hatch their eggs in October: the yolks of the yellow, the holey, and jumping penguins, are yellow; but of the red penguins, it is red. All their eggs are good nourifhing food, and a great refrefhment to the feamen; but the flefh of thefe animals is coarfe, fifhy, and wholly unfit to eat.

The

The only beaſt on theſe iſlands is a fox, very nearly reſembling the Engliſh fox; it is now very ſhy and ſcarce to be got.

Of birds. There are three ſorts of wild geeſe: the mountain gooſe is ſomewhat larger than a Muſcovy duck; feed always on the mountains, is pleaſant taſted, and preferable to the other ſorts, but is ſcarce. Its plumage on the back is ſpeckled with brown and black, of a greeniſh hue, and towards the neck turns of a gloſſy beautiful golden colour; the breaſt is coloured like a pheaſant. The other gooſe feeds in the vallies on the wild cranberries and graſs, and is as large as a tame gooſe; the gander is black and white ſpeckled; the gooſe is almoſt like the mountain gooſe, but darker and not ſo beautiful. Theſe are good food in general; but beſt and fatteſt in February, March, and April. Of the ſea-gooſe; the gander is white, the gooſe mottled, black and white; they feed always on the ſea-ſhore, and are ſcarcely eatable. Wild ducks, widgeon, teal, and the ſhelldrakes, are the ſame as in Europe. But here is a ſpecies of ducks, called the loggerhead, from its large head. They have ſhort wings, are unable to fly, and only ſwim and flap along on the water at an extraordinary rate. When driven aſhore with boats they run faſt, but ſoon ſquat down and are eaſily caught; they are eatable, but are but indifferent food: they are of a dark brown dirty colour. Snipes are plenty, and ſo exceedingly tame that we could ſhy at them with ſticks, and get a diſh whenever we wanted. Of ſmall birds there are ſeveral ſorts; the red breaſt, ſpeckled

on the back like a partridge; the yellow breaſt; the white throat; the quaker, from its plumage being of the colour thoſe people wear; the ſparrow; tom-tit; linnets, and a bird like a goldfinch. Hawks are numerous; the eagle, the goſhhawk, the ſparrow and the common hawk. Of every kind our crew ate, and found them very good and nouriſhing; owls there are not numerous.

Over the ſeveral iſlands is a ſurprizing ſpecies of vegetation, which I know not what to call. It reſembles, at firſt view, our molehills in the marſhy grounds in England. It is circular; ſometimes ſix feet round, ſometimes leſs. From the ſurface oozes out a gum in round blebs, of the ſmell and taſte of balſam *capavia*. The body of theſe hills is formed within by a number of ſmall ſubſtances, like the cones of pines. The outſide is cruſted over with dark green ſmall leaves, running into each other, and cemented as if with glue. I opened ſeveral, and found that no vermin formed them; but there actually was a kind of vegetation; and yet the wild cranberries vegetate on them when the ſeed is lodged on them. The balſam I brought to England, and it is now on trial by an eminent ſurgeon.

Fern abounds, but is of a weaker ſort than ours in Europe. We tried the furze-ſeed, and it came up; but ſo weak and poor that it would never increaſe or thrive. We found the ſeaſon for ſowing all kinds of garden ſeeds was about three weeks later than directed in the ſpring or fall by MILLER, remembering to reverſe his months, calling September, March, &c.; but all kind of culinary

herbs and roots came to as great perfection as in England, and in great plenty, only we were forced to ſhelter every bed in the garden, by a good ſod wall, from the S.W. and S.E. winds as much as poſſible, otherwiſe our labour would have been in vain.

The latter end of September or beginning of October, the ſea birds begin to come on ſhore to build neſts and lay. The firſt which appear are the albatroſs, which are about the ſize of a large gooſe, quite white, except their wings, which are a dark brown; the bills are of a dirty yellow, about three inches long; very ſtrong and the edges ſharp as a knife, hooked at the point; they breathe hard through two ſmall holes in the bill cloſe to the head, and frequently make a ſound like a trumpet which children buy at fairs. Their wings are very long and narrow, with four joints in each wing, and extend ten or twelve feet from tip to tip. Their feet are webbed, very thin, have three claws; on the outer claw are four joints, the middle three, and the inner one. When they come to their towns, as we called their neſting-places, it is by hundreds. They ſet very tame, and continue one or other continually ſounding their bills. They never move off their neſts let what will approach, and we ſhoved them off whenever we wanted their eggs. The egg is much larger than a gooſe's. The yolk is yellow; the white never boils hard, and always continues as clear as iſinglaſs; our crew found them a good refreſhment, though I thought them very ſtrong. The neſts of theſe birds are made on the ground with earth; are round, about

one foot high, and dented at top. While the hen ſits, the male keeps conſtantly on the wing, and morning and evening returns with food to her. As ſoon as theſe birds have hatched, and the young ones are able to leave their neſts, the jumping penguins repair to the neſts and occupy them. The young albatroſs's remain among them while the old ones go and ſeek food, with which they regularly return morning and evening. The ſeaſon for every ſpecies of birds, wild and tame, laying and hatching is from September to December or January, and as all the eggs are very eatable, navigators touching at theſe iſlands in thoſe months will meet great refreſhment. In thoſe three months we never meddled with the land geeſe, as they were breeding and could not be good.

The ſoil is in general boggy, barren, and rocky; but affords good paſturage in the vallies, and level ſpots for ſheep and goats, and would for cattle, which might be out all winter; for that ſeaſon is more remarkable for its mildneſs than in the ſame degree of Northern latitude. The ſummer is as remarkably cold, and both proceeds from the prevailing winds; in the winter the N. and N.E. winds are frequent, which brings warm, mild, moiſt weather. In the ſummer, the S. and S.W. and S.E. prevails, which are cold, ſharp, and blighting; but in general, throughout the year, there is very little difference in the weather, but moſtly cold. The thermometer ſcarce ever exceeds 64° in the warmeſt days, and very ſeldom in winter is below the freezing point, though I have ſeen it 20° below freezing; but that did not continue long,

 nor

nor does the ſnow continue in the plains or vallies a week together, or froſt laſt ſo long; but the weather in winter is perpetually changing, the ſnow lies on the hills for nine months.

There is a great plenty, and ſome variety of moſs on all the iſlands, and moſt of it when wet with water dyes of a brick-duſt red. I tried it with other liquids, but found it ſtill the ſame; ſo I believe it can be of no uſe.

The coaſts of theſe iſlands abounds with whales of the ſpermaceti kind; the iſlands with innumerable ſeals and ſea-lions, from whence a valuable fiſhery might, if thought proper, be carried on (c). The paſſage out is twelve weeks; the ſame home. Ships might be loaded with oil ready made in ſix and eight weeks, and the price of that article greatly reduced.

Theſe are all the remarks I made while I commanded on that barren, dreary, deſolate, boggy, rocky ſpot, in 1774.

(c) The year on which Lieutenant CLAYTON left the Falkland Iſlands, there were ten veſſels from North America employed in whale fiſhing; and it is ſuppoſed, that the voyages anſwered very well, though in going out they commonly proceed as far to the Eaſtward as the Cape de Verde Iſlands.

VI. *Short*

VI. *Short and easy Theorems for finding, in all cases, the Differences between the Values of Annuities payable Yearly, and of the same Annuities payable Half-yearly, Quarterly, or Momently. By the Rev.* Richard Price, *D. D. F. R. S. In a Letter to Sir* John Pringle, *Bart. P. R. S.*

R. Nov. 9, 1775. THE values of annuities, as given in all the common tables, suppose them paid yearly. But it is well known, that generally they are paid half-yearly, and sometimes quarterly; and that this is a circumstance which always adds to their value. The difference between the values of annuities, according as they are paid in these different ways, I have seen no where stated with accuracy; and therefore, I have thought that the following attempt to do this may be of some use.

Annuities are of two sorts. They are either payable certainly or conditionally. Of the former sort are all annuities which are payable at fixed times, without depending on any contingency. Of the latter sort are all annuities on lives. I will first consider the first sort of annuities.

Let r denote the interest of 1£. for a year; and n the term or number of years during which any anuuity is to be

be paid. Let P denote the value of the perpetuity, or the quotient arising from dividing 1 £. by its interest for a year. Let y denote the value of an annuity for n years, supposing it to be paid yearly; h its value, payable half-yearly; q its value, payable quarterly; and m its value, payable momently.

THEOREM I.

$$y = P - \frac{1}{r \times \overline{1+r}|^{n}}.$$

THEOREM II.

$$h = P - \frac{1}{r \times \overline{1+\frac{r}{2}}|^{2n}}.$$

THEOREM III.

$$q = P - \frac{1}{r \times \overline{1+\frac{r}{4}}|^{4n}}.$$

THEOREM IV.

$m = P - \frac{1}{rN}$. where N denotes the number which hath rn for its hyperbolic logarithm, and $rn \times 0.43429448$ for its logarithm in BRIGG's system.

EXAMPLE.

Let the rate of interest be 4 *per cent.* and the term 5 years, and consequently $r = 0,04$. $n = 5$, P $= 25$.

Then,

Then, $y=4.4518$
$h=4.4913$
$q=4.5120$
$m=4.5415$

EXAMPLE II.

Let the rate of intereſt be the ſame, and the term for which the annuity is payable 25 years.

Then, $y=15.6220$
$h=15.7118$
$q=15.7694$
$m=15.801$

EXAMPLE III.

Intereſt being the ſame, let the term be 50 years.

Then, $y=21.4822$
$h=21.5491$
$q=21.582$
$m=21.616$

EXAMPLE IV.

Intereſt being the ſame, let the term be 100 years.

Then, $y=24.505$
$h=24.523$
$q=24.532$
$m=24.542$

In the foregoing theorems it may be obferved, that the *ratio* to one another, of the values of annuities payable yearly, half-yearly, quarterly, and momently, is greateft when *n* is leaft; that it decreafes continually as *n* increafes, till at laft it vanifhes, when *n* becomes infinite or the annuity is a perpetuity. Agreeably to this it appears, in the examples I have given, that the values in the firft example differ more from one another in proportion than the values in the fecond example; and that thefe alfo differ more than the values in the third; and that in the laft example all the values are nearly the fame.

Thefe values computed by Mr. DE MOIVRE's rules in his Treatife on Life-annuities, p. 86. and 124, &c. come out greater when *n* exceeds, and lefs when *n* falls fhort of 15 or 20 years. But thofe rules fuppofe the half-yearly and quarterly interefts of money to be lefs than half or a quarter of the yearly intereft. For inftance; the value of an annuity of 1 £. payable half-yearly and quarterly for 50 years is, according to Mr. DE MOIVRE's rules, 21,699 and 21,772, or a 99th part and 74th part more than the value of the fame annuity payable yearly, fuppofing money improved at 4 *per cent.* when the annuity is paid yearly; and at £. 1,98 *per cent.* when it is paid half-yearly; and at 0,985 *per cent.* when it is paid quarterly: That is; fuppofing money improved at a rate of half-yearly or quarterly intereft, which, inftead of being a half or a quarter of the yearly intereft, is only that half-

half-yearly or quarterly payment which, in consequence of being laid up and improved at compound interest, will in a year amount to the sum that makes the yearly interest. It is obvious that this cannot be the proper method of computing these values. But not to insist on this; I will next state the different values of the second sort of annuities; or of life-annuities, according as they are supposed to be payable yearly, half-yearly, quarterly, or momently.

Let r as before be the interest of 1 £. for a year; n the complement of a given life[(a)]; y, h, q, and m, the values respectively of an annuity certain for n years payable yearly, half-yearly, quarterly, or momently; P the perpetuity; Y the present value of an annuity on a life whose complement is n, payable yearly; H the value of the same annuity payable half-yearly; and Q and M the values of the same annuity payable quarterly and momently.

(a) The complement of a life is, in Mr. DE MOIVRE's hypothesis, the number of years it wants of 86. In all other cases, it is double the expectation of a life; that is, it is double the quotient (diminished by ½ unity) arising from dividing the sum of all the living in a table of observations from the age (inclusive) of the given life to the extremity of life, by the number of the living at that age. See Essay I. in my Treatise on Reversionary Payments.

Then,

Then, $Y = P - \frac{1+r}{rn} \times y.$

$$H = P - \frac{1+\frac{r}{2}}{nr} \times h.$$

$$Q = P - \frac{1+\frac{r}{4}}{nr} \times q.$$

$$M = P - \frac{m}{nr}.$$

EXAMPLE I.

Let the life be ſuppoſed of the age of 36. The complement of ſuch a life is 50, according to Mr. DE MOIVRE's hypotheſis; and alſo very nearly, according to the Breſlaw and the Northampton tables of obſervations. Therefore, n will be 50. Let the rate of intereſt be 4 *per cent*, or $r=0{,}04$. $P=25$. $y=21{,}482$. $h=21{,}549$. $q=21{,}582$. $m=21{,}616$. See p. 111.

Therefore, $Y = 25 - \frac{1{,}04}{50 \times 0{,}04} \times 21{,}482 = 13{,}829$

$$H = 25 - \frac{1{,}02}{50 \times 0{,}04} \times 21{,}549 = 14{,}010$$

$$Q = 25 - \frac{1{,}01}{50 \times 0{,}04} \times 21{,}582 = 14{,}101$$

$$M = 25 - \frac{21{,}616}{50 \times 0{,}04} = 14{,}191$$

EXAMPLE II.

Let the life be ſuppoſed of the age of 61. The complement of this life is 25 by Mr. DE MOIVRE's hypotheſis

fis and the Northampton table of obfervations. Therefore, intereft fuppofed at 4 *per cent.*

$$Y = 25 - \frac{1,04}{25 \times 0,04} \times 15,622 = 8,753$$

$$H = 25 - \frac{1,02}{25 \times 0,04} \times 15,712 = 8,973$$

$$Q = 25 - \frac{1,01}{25 \times 0,04} \times 15,769 = 9,072$$

$$M = 25 - \frac{15,801}{25 \times 0,04} = 9,199$$

The different values, given by thefe theorems, of life-annuities payable yearly, half-yearly, and quarterly, fuppofe nothing to be due to an annuitant for that year, half-year, or quarter, in which he fhall happen to die. If, on the contrary, he is to be entitled to fuch part of the annuity as fhall be proportioned to the time which fhall happen to intervene between his death and the time when the payment immediately preceding his death became due; or in other words, if the annuity is an annuity fecured by land, $\frac{y}{2n}$ muft be added to the firft theorem in order to obtain the value of fuch an annuity payable yearly. And in like manner, $\frac{h}{4n}$ muft be added to the fecond theorem to obtain the value of the fame annuity payable half-yearly: and $\frac{q}{8n}$ to the third theorem, to obtain its value payable quarterly.

The value, therefore, in the firft example, of an annuity payable yearly on a life aged 36 being 13,829; its value, if fecured by land, or to be enjoyed to the laft moment of

 life,

life, will be $13,829+\frac{21,482}{100}=14,043$. If ſecured by land and payable half-yearly, its value will be $14,010+\frac{21,549}{200}=14,117$. If ſecured by land and payable quarterly, its value will be $14,101+\frac{21,582}{400}=14,155$. The like values in the ſecond example are 9,065, 9,130, and 9,151.

Life-annuities payable monthly or weekly may be conſidered as of the ſame value with annuities payable momently; and it is evident, that they muſt be enjoyed nearly to the laſt moment of life.

From theſe rules and examples it may be gathered, that the difference between the values of annuities on lives payable yearly, half-yearly, quarterly, and momently, increaſes continually with the ages; but, if not ſecured by land, this difference can never be ſo great as a quarter of a year's purchaſe in the caſe of annuities payable yearly and half-yearly; three-eighths of a year's purchaſe in the caſe of annuities payable yearly and quarterly; and half a year's purchaſe in the caſe of annuities payable yearly and momently.

Mr. SIMPSON, in his Treatiſe on the Doctrine of Life-annuities, p. 78. and in his Select Exerciſes, p. 283. hath given a quarter of a year's purchaſe as the addition always to be made to the value of a life-annuity payable yearly, in order to obtain its value payable half-yearly; and three-eighths of a year's purchaſe, if its value payable quarterly is required. But it appears, that theſe are too large additions; and, whatever be the rate of intereſt,

or the number of lives, a fifth of a year's purchafe will be generally more than a fufficient addition, if the value of the annuity is defired payable half-yearly; and three-tenths of a year's purchafe, if the value of the annuity is defired payable quarterly. Mr. DE MOIVRE's rules, in p. 85 of his Book on Life-annuities, for finding the values of life-annuities payable half-yearly and quarterly from their values payable yearly, are ftill lefs correct; for they fuppofe the difference between thefe values the fame, whether the annuities are life-annuities, or annuities certain.

Mr. DODSON, in the firft queftion in the third volume of his Mathematical Repofitory, hath given a rule for finding the value of an annuity fecured by land and payable yearly, which coincides with that here given; and Mr. DE MOIVRE, in p. 338. of his Treatife on the Doctrine of Chances, hath given a theorem for this purpofe, which alfo brings out nearly the fame anfwers. But Mr. SIMPSON, in prob. I. p. 323. of his Select Exercifes, makes the excefs of the value of fuch an annuity above the value of an annuity payable yearly, but not fecured by land, double to the fame excefs derived from Mr. DODSON's and Mr. DE MOIVRE's rules. The truth is, that Mr. DODSON's rule gives the exact value; and that Mr. SIMPSON's problem gives the value, not of an annuity fecured by land and payable yearly, but of an annuity fecured by land and payable momently; and alfo, that his method of folution implies a rate of intereft fomewhat lefs when the annuity is payable momently than when it is payable yearly.

But

But to prevent all perplexity on this ſubject, I will ſubjoin the following inveſtigations, which will be eaſily underſtood by thoſe who are acquainted with the common methods of calculating the values of life-annuities.

Let r, as before, be the intereſt of £. 1 for a year. Then the preſent value of £. 1 payable at the end of one year, two years, three years, &c. will be $\frac{1}{1+r}$, $\frac{1}{\overline{1+r}|^2}$, $\frac{1}{\overline{1+r}|^3}$, &c. reſpectively. And the preſent value of an annuity certain for n years payable yearly is the ſum of this ſeries continued to n terms (b), or $\frac{1}{r}-\frac{1}{r\times\overline{1+r}|^n}=\text{P}-\frac{1}{r\times\overline{1+r}|^n}=y$.

In like manner, the preſent value of half £. 1 (that is of 10 *s*.=£.0,5) payable at the end of half a year, a year, a year and a half, &c. reckoning half-yearly intereſt at half the annual intereſt, is $\frac{0{,}5}{1+\frac{r}{2}}$, $\frac{0{,}5}{\overline{1+\frac{r}{2}}|^2}$, $\frac{0{,}5}{\overline{1+\frac{r}{2}}|^3}$, &c. And the preſent value of an annuity certain payable half-yearly for n years, each payment to be half the yearly payment, is the ſum of this ſeries continued to $2n$ terms; or,

$$\frac{0{,}5}{\frac{r}{2}}-\frac{0{,}5}{\frac{r}{2}\times\overline{1+\frac{r}{2}}|^{2n}}=\frac{1}{r}-\frac{1}{r\times\overline{1+\frac{r}{2}}|^{2n}}=\text{P}-\frac{1}{r\times\overline{1+\frac{r}{2}}|^{2n}}=b.$$

(b) In the poſtſcript it will be proved, that the ſum of n terms of the ſeries $\frac{1}{a}+\frac{1}{a^2}+\frac{1}{a^3}+\frac{1}{a^4}$, &c. is $\frac{1}{a-1}-\frac{1}{a^n\times\overline{a-1}}$. Subſtitute $1+r$ for a, and it will appear, that the ſum of n terms of the ſeries $\frac{1}{1+r}+\frac{1}{\overline{1+r}|^2}+\frac{1}{\overline{1+r}|^3}$, &c. is $\frac{1}{r}-\frac{1}{r\times\overline{1+r}|^n}$.

By

By the ſame ſteps it will appear, that the preſent value of an annuity certain for n years to be received in quarterly payments, each a quarter of the annual payment, is $\frac{0{,}25}{\frac{1}{4}r} - \frac{0{,}25}{\frac{1}{4}r \times \overline{1+\frac{r}{4}}|^{4n}} = \text{P} - \frac{1}{r \times \overline{1+\frac{r}{4}}|^{4n}} = q$. And alſo, that the preſent value of an annuity certain for n years, to be received in momently payments, each the ſame proportional part of the yearly payment that the moment is of the year, muſt be $\text{P} - \frac{1}{r \times \overline{1+\frac{r}{1000, \&c.}}|^{1000, \&c.\, n}}$. But, by the binomial theorem, $\overline{1+\frac{r}{1000, \&c.}}|^{1000, \&c.\, n} = 1 + rn + \frac{r^2n^2}{2} + \frac{\cdot}{2\times 3} + \frac{r^4n^4}{2\times 3\times 4}$, &c. which ſeries approximates indefinitely to that number of which rn is the hyperbolic logarithm, by prob. 1. ſect. XI. vol. II. of Mr. SIMPSON's Fluxions; or by prop. 1. p. 40. of his Treatiſe on Trigonometry. Therefore, $\text{P} - \frac{1}{r \times \overline{1+\frac{r}{1000, \&c.}}|^{1000, \&c.\, n}} = \text{P} - \frac{1}{r\text{N}} = \text{M}$, as explained before. See p. 110.

If the value of an annuity of £. 1 for n years is required payable half-yearly, and the half-yearly intereſt of £. 1, inſtead of being half the yearly intereſt (or $\frac{r}{2}$), is ſuppoſed to be $\overline{1+r}|^{\frac{1}{2}} - 1$; the anſwer will be $\frac{0{,}5}{\overline{1+r}|^{\frac{1}{2}}} + \frac{0{,}5}{1+r} + \frac{0{,}5}{\overline{1+r}|^{1\frac{1}{2}}} + \frac{0{,}5}{\overline{1+r}|^{2}}$, &c. continued to $2n$ terms =

=

$= \frac{0,5}{\overline{1+r}|^{\frac{1}{2}}-1} - \frac{0,5}{\overline{1+r}|^{n} \times \overline{1 \times r}|^{\frac{1}{2}}-1} = \overline{1 - \frac{1}{\overline{1+r}|^{n}}} \times \frac{1}{2 \times \overline{1+r}|^{\frac{1}{2}}-2}$; which value is to $1 - \frac{1}{\overline{1+r}|^{n}} \times \frac{1}{r}$ (the value of the ſame annuity payable yearly ſuppoſing the yearly intereſt of £. 1 to be r) as $\frac{\frac{1}{2}}{\overline{1+r}|^{\frac{1}{2}}-1}$ to $\frac{1}{r}$, agreeably to Mr. DE MOIVRE's deduction in his Treatiſe on Life-annuities, p. 125. fourth edit. This implying, in the caſe of annuities payable half-yearly, a ſmaller intereſt than half the yearly intereſt (for $\overline{1+r}|^{\frac{1}{2}}-1$ is leſs than $\frac{r}{2}$) gives the difference between their value and the value of annuities payable yearly, greater than the truth.

But to return to the inveſtigation of the theorems in the former part this paper.

Let us again call P the perpetuity, and y the value of an annuity certain for n years and payable yearly; it is well known that the value of £. 1 payable yearly on a life whoſe complement is n is (ſuppoſing an equal decrement of life) $\frac{n-1}{n \times \overline{1+r}} + \frac{n-2}{n \times \overline{1+r}|^{2}} + \frac{n-3}{n \times \overline{1+r}|^{3}}$, &c. continued to n terms(c) $= \text{P} - \frac{1+r}{nr} \times y = \text{Y}$.

In

(c) See Mr. DE MOIVRE's Treatiſe on Life-annuities, p. 99. fourth edit. Or his Doctrine of Chances, p. 311. third edition. Or Mr. DODSON's Mathematical Repoſitory, vol. II. p. 137. Or Mr. SIMPSON on Annuities and Reverſions, p. 14. In conſulting theſe writers, care ſhould be taken to remember, that they uſe r to denote the principal and intereſt of £. 1 for a year; whereas it hath been

In like manner, ſuppoſing money improved at an half-yearly intereſt equal to half the yearly intereſt, or to $\frac{r}{2}$, the value of the ſame annuity payable half-

been moſt convenient for me in theſe obſervations to make r ſtand only for the intereſt. In theſe writers, therefore, r ſignifies the ſame with $1+r$ in this paper; and $r-1$ the ſame with r.

It is ſaid above, that the value of an annuity payable yearly, on a life whoſe complement is n, is $\frac{n-1}{n\times\overline{1+r}}+\frac{n-2}{n\times\overline{1+r}|^{2}}+\frac{n-3}{n\times\overline{1+r}|^{3}}$, &c. continued to n terms. This expreſſion is equal to $\frac{n}{n\times\overline{1+r}}+\frac{n}{n\times\overline{1+r}|^{2}}+\frac{n}{n\times\overline{1+r}|^{3}}$, &c. (n) $-\frac{1}{n}\times\frac{1}{1+r}+\frac{2}{\overline{1+r}|^{2}}+\frac{3}{\overline{1+r}|^{3}}$, &c. (n). But $\frac{n}{n\times\overline{1+r}}+\frac{n}{n\times\overline{1+r}|^{2}}+\frac{n}{n\times\overline{1+r}|^{3}}$ &c. $\left(=\frac{1}{1+r}+\frac{1}{\overline{1+r}|^{2}}+\frac{1}{\overline{1+r}|^{3}}\right.$, &c.$\left.\right)=\frac{1}{r}-\frac{1}{r\times\overline{1+r}|^{n}}=y$ (ſee p. 118.) Alſo, by a theorem which will be demonſtrated in the poſtſcript, and putting a for any given quantity, $\frac{1}{a}+\frac{2}{a^{2}}+\frac{3}{a^{3}}$, &c. continued to n terms $=\frac{a}{\overline{a-1}|^{2}}-\frac{n}{a^{n}}\times\frac{1}{a-1}-\frac{1}{a^{n}}\times\frac{a}{\overline{a-1}|^{2}}$. Therefore, if $1+r$ is ſubſtituted for a, and y for $\frac{1}{r}-\frac{1}{r\times\overline{1+r}|^{n}}$, the ſum (multiplied by $\frac{1}{n}$) of n terms of the ſeries $\frac{1}{1+r}+\frac{2}{\overline{1+r}|^{2}}+\frac{3}{\overline{1+r}|^{3}}$, &c. will come out $\frac{1+r}{nr}\times y-\frac{1}{r}\times\frac{1}{\overline{1+r}|^{n}}$; or $\frac{1+r}{nr}\times y+y-\frac{1}{r}$. Therefore, the ſeries $\frac{1}{n}\times\overline{\frac{1}{1+r}+\frac{2}{\overline{1+r}|^{2}}+\frac{3}{\overline{1+r}|^{3}}}$, &c. continued to n terms and ſubtracted from the ſeries $\frac{1}{1+r}+\frac{1}{\overline{1+r}|^{2}}+\frac{1}{\overline{1+r}|^{3}}$, &c. continued to n terms; that is, the value of the life will be $y-\overline{\frac{1+r}{nr}\times y+y-\frac{1}{r}}=\frac{1}{r}-\frac{1+r}{nr}\times y=P-\frac{1+r}{nr}\times y=Y$.

 yearly

yearly, is $\frac{1}{2}\times\frac{n-\frac{1}{2}}{n\times 1+\frac{r}{2}}+\frac{n-1}{n\times\left(1+\frac{r}{2}\right)^{2}}+\frac{n-\frac{3}{2}}{n\times\left(1+\frac{r}{2}\right)^{3}}$, &c. continued to $2n$ terms $=\frac{1}{2}\times\frac{n}{n\times 1+\frac{r}{2}}+\frac{n}{n\times\left(1+\frac{r}{2}\right)^{2}}+\frac{n}{n\times\left(1+\frac{r}{2}\right)^{3}}$, &c. continued to $2n$ terms $-\frac{1}{2}\times\overline{\frac{\frac{1}{2}}{n\times 1+\frac{r}{2}}+\frac{1}{n\times\left(1+\frac{r}{2}\right)^{2}}+\frac{\frac{3}{2}}{n\times\left(1+\frac{r}{2}\right)^{3}}}$, &c. continued to $2n$ terms. But the sum of the first of these two series, or of $\frac{1}{2}\times\frac{n}{n\times 1+\frac{r}{2}}+\frac{n}{n\times\left(1+\frac{r}{2}\right)^{2}}$, &c. $\left(=\frac{1}{2}\times\frac{1}{1+\frac{r}{2}}+\frac{1}{\left(1+\frac{r}{2}\right)^{2}}\right.$, &c.$)$ is b, see p. 118. And the sum of the second series is the same with half the sum of the series $\frac{1}{2n}\times\overline{\frac{1}{1+\frac{r}{2}}+\frac{2}{\left(1+\frac{r}{2}\right)^{2}}+\frac{3}{\left(1+\frac{r}{2}\right)^{3}}}$, &c. $(2n)$. But by the theorem mentioned in the last note, the sum of n terms of the series $\frac{1}{a}+\frac{2}{a^{2}}+\frac{3}{a^{3}}$, &c. is $\frac{a}{(a-1)^{2}}-\frac{n}{a^{n}}\times\frac{1}{a-1}-\frac{1}{a^{n}}\times\frac{a}{(a-1)^{2}}$. Therefore, if $1+\frac{r}{2}$ is substituted for a, $2n$ for n, and b for $\frac{1}{r}-\frac{1}{r\times\left(1+\frac{r}{2}\right)^{2n}}$, the sum of the second series (that is, of $\frac{1}{2}\times\frac{1}{2n}\times\frac{1}{1+\frac{r}{2}}+\frac{2}{\left(1+\frac{r}{2}\right)^{2}}+\frac{3}{\left(1+\frac{r}{2}\right)^{3}}$, &c. $(2n)$) will come out $\frac{1+\frac{r}{2}}{nr}\times b-\frac{1}{r}\times\frac{1}{\left(1+\frac{r}{n}\right)^{2n}}$, or $\frac{1+\frac{r}{2}}{nr}\times b+b-\frac{1}{r}$.

Therefore,

Therefore, the ſecond ſeries ſubtracted from the firſt, leaves $\frac{1}{r}-\frac{1+\frac{r}{2}}{nr}\times h=\text{P}-\frac{1+\frac{r}{2}}{nr}\times h=\text{H}$, agreeably to the ſecond theorem in p. 114.

By reaſoning in the ſame way it may be eaſily found, that $\text{Q}=\text{P}-\frac{1+\frac{r}{4}}{nr}\times q$; and $\text{M}=\text{P}-\frac{1+\frac{r}{1000,\&c.}}{nr}\times m=\text{P}-\frac{m}{nr}$, agreeably to the third and fourth theorems in p. 114.

Theſe theorems, I have ſaid, ſuppoſe that an annuitant is entitled to no payment for that year, half-year, or quarter, in which he dies. If, on the contrary, he is to be intitled, when he dies, to ſuch a part of the yearly, half-yearly, or quarterly payment as ſhall bear the ſame proportion to the ſaid payments reſpectively, as the intermediate time between the laſt payment and his death bears to the whole year, half-year, or quarter; in this caſe, ſuppoſing the annuity payable yearly, it is evident, ſince there is the ſame chance for his dying in one half of any year as in the other, that he will have an expectation of half a year's payment more than he would be otherwiſe intitled to. But the value of half £. 1 to be paid at the death of a perſon whoſe complement of life is n, is $\frac{1}{2}\times\frac{1}{n\times\overline{1+r}}+\frac{1}{2}\times\frac{1}{n\times\overline{1+r}|^2}+\frac{1}{2}\times\frac{1}{n\times\overline{1+r}|^3}$, &c. continued to n terms (d) $=\frac{y}{2n}$.

(d) P. 118.

In like manner, a perſon who enjoys an annuity ſecured by land, payable half-yearly, will have an expectation of a quarter of a year's payment more than he could be otherwiſe intitled to; the value of which is $\frac{1}{4n} \times \frac{1}{1+\frac{r}{2}} + \frac{1}{\overline{1+\frac{r}{2}}|^2} + \frac{1}{\overline{1+\frac{r}{2}}|^3}$, &c. continued to $2n$ terms $= \frac{b}{4n}$.

By the ſame reaſoning it will appear, that $\frac{q}{8n}$ is the addition to be made to the value of an annuity payable quarterly, in order to obtain its value when ſecured by land.

P O S T S C R I P T.

IN the note p. 121. the expreſſion $\frac{1}{a-1} - \frac{1}{a^n} \times \frac{1}{a-1}$ is given as the ſum of n terms of the ſeries $\frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3} + \frac{1}{a^4}$, &c. to $\frac{1}{a^n}$, and the expreſſion $\frac{a}{\overline{a-1}|^2} - \frac{n}{a^n} \times \frac{1}{a-1} - \frac{1}{a^n} \times \frac{a}{\overline{a-1}|^2}$, is given as the ſum of n terms of the ſeries $\frac{1}{a} + \frac{2}{a^2} + \frac{3}{a^3} + \frac{4}{a^4}$, &c.

The following inveſtigation of theſe theorems being very eaſy, will not, perhaps, be unacceptable to thoſe who have ſtudied this ſubject.

Put $A = \frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3} + \frac{1}{a^4}$, &c. $\frac{1}{a^n}$. $B = \frac{1}{a} + \frac{2}{a^2} + \frac{3}{a^3} + \frac{4}{a^4}$, &c. $\frac{n}{a^n}$.

Then $A \times a = 1 + \frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3}$, &c. to $\frac{1}{a^{n-1}}$,

and $A \times a - 1 + \frac{1}{a^n} = \frac{1}{a} + \frac{1}{a^2} + \frac{1}{a^3}$, &c. to $\frac{1}{a^{n-1}} + \frac{1}{a^n} = A$,

and

$$\text{and } A\times a - A (= A\times\overline{a-1}) = 1 - \frac{1}{a^n}.$$

Therefore, $A=\frac{1}{a-1}-\frac{1}{a^n}\times\frac{1}{a-1}$, which is the firſt theorem.

$$\text{Again, } A\times a = 1+\frac{1}{a}+\frac{1}{a^2}+\frac{1}{a^3}, \text{ \&c. to } \frac{1}{a^{n-1}},$$

$$\text{and } B\times a = 1+\frac{2}{a}+\frac{3}{a^2}+\frac{4}{a^3}, \text{ \&c. to } \frac{n}{a^{n-1}}.$$

$$\text{Therefore, } B\times a - A\times a = \frac{1}{a}+\frac{2}{a^2}+\frac{3}{a^3}, \text{ \&c. to } \frac{n-1}{a^{n-1}}.$$

To both ſides of the laſt equation add $\frac{n}{a^n}$, and it will appear, that

$$B\times a - A\times a + \frac{n}{a^n} = \frac{1}{a}+\frac{2}{a^2}+\frac{3}{a^3}+\frac{4}{a^4}+, \text{ \&c. to } \frac{n-1}{a^{n-1}}+\frac{n}{a^n}=B.$$

Therefore, $B\times a - B = B\times\overline{a-1} = A\times a - \frac{n}{a^n}$; and $B=\frac{A\times a}{a-1}-\frac{n}{a^{n+1}-a^n}$.

For A, in this laſt equation, ſubſtitute its equal, or $\frac{1}{a-1}-\frac{1}{a^n}\times\frac{1}{a-1}$, and the reſulting equation will be $\frac{a}{\overline{a-1}|^2}-\frac{n}{a^n}\times\frac{1}{a-1}-\frac{1}{a^n}\times\frac{a}{\overline{a-1}|^2}=B$, which is the ſecond theorem.

When n is infinite, all but the firſt terms in both theſe theorems vaniſh; and therefore, $\frac{1}{a-1}$ is the ſum of the ſeries $\frac{1}{a}+\frac{1}{a^2}+\frac{1}{a^3}$, &c. continued infinitely; and $\frac{a}{\overline{a-1}|^2}$ is the ſum of the ſeries $\frac{1}{a}+\frac{2}{a^2}+\frac{3}{a^3}$, &c. continued infinitely.

By

By a like deduction, putting

$$C=\frac{1}{a}+\frac{2\times 2}{a^2}+\frac{3\times 3}{a^3}+\frac{4\times 4}{a^4}, \text{ \&c. to } \frac{n^2}{a^n},$$

and $D=\frac{1}{a}+\frac{2\times 2\times 2}{a^2}+\frac{3\times 3\times 3}{a^3}+\frac{4\times 4\times 4}{a^4}$, &c. to $\frac{n^3}{a^n}$, it may be found that $C=\frac{A+2B+1}{a-1}-\frac{\overline{n+1}|^2}{a^{n+1}-a^n}$, and $D=\frac{A+3B+3C+1}{a-1}-\frac{\overline{n+1}|^3}{a^{n+1}-a^n}$.

And consequently, substituting the values of A and B, that

$$C=\frac{a^2+a}{\overline{a-1}|^3}-\frac{n^2}{a^n}\times\frac{1}{a-1}-\frac{2an}{a^n}\times\frac{1}{\overline{a-1}|^2}-\frac{a^2+a}{a^n}\times\frac{1}{\overline{a-1}|^3}.$$

And, substituting the values of A, B, C, that

$$D=\frac{a^3+4a^2+a}{\overline{a-1}|^4}-\frac{n^3}{a^n}\times\frac{1}{a-1}-\frac{3an^2}{a^n}\times\frac{1}{\overline{a-1}|^2}-\frac{3a^2n+3an}{a^n}\times\frac{1}{\overline{a-1}|^3}-\frac{a^3+4a^2+a}{a^n}$$

$\times\frac{1}{\overline{a-1}|^4}$. Or, since all but the first terms in these expressions vanish when n is infinite, that the sum of the series $\frac{1}{a}+\frac{4}{r}+\frac{9}{a^3}$, &c. continued infinitely is $\frac{a^2+a}{\overline{a-1}|^3}$; and that the sum of the series $\frac{1}{a}+\frac{8}{a^2}+\frac{27}{a^3}+\frac{64}{a^4}$, &c. continued infinitely is $\frac{a^3+4a^2+a}{\overline{a-1}|^4}$.

These are all the theorems necessary for calculating the values of annuities on single lives, and on any two or three joint lives, upon the hypothesis of an equal decrement of life.

Supposing r the interest of £. 1 for a year, the sum of n terms of the series $\frac{1}{1+r}+\frac{1}{\overline{1+r}|^2}+\frac{1}{\overline{1+r}|^3}$, &c. is the present value of an annuity certain for n years; and

$\frac{1}{1+r}$

$\frac{1}{1+r}+\frac{2}{\overline{1+r}|^2}+\frac{3}{\overline{1+r}|^3}+\frac{4}{\overline{1+r}|^4}$, (continued to n terms) is the preſent value of an annuity certain beginning with £. 1, and increaſing to £. 2 the ſecond year, to £. 3 the third year, &c.

If this laſt annuity is not an annuity certain for a given term, but a life-annuity, the value of it (ſuppoſing n the complement of life, A the value of an annuity certain for n years, G the value of two equal joint lives whoſe common complement is n, P the perpetuity, and p the value of £. 1 to be received at the end of n years) will be $\overline{A-G}\times n+n\times p\times P-A\times P\times\overline{1+r}$.

EXAMPLES.

Let the term be forty-one years, and the rate of intereſt 4 per cent.

The value of an annuity of £. 1 certain for this term is £. 20.

The value of an annuity certain for the ſame term, and beginning with £. 1 at the end of the firſt year, but increaſing to £. 2 at the end of the ſecond year, to £. 3 at the end of the third year, and ſo on till it becomes £. 41 at the end of the forty-firſt year, is (by the ſecond theorem, putting $1+r$, or 1,04 for a) £. 214 10*s*.

The value of an annuity increaſing at this rate without end is £. 650.

If

If the annuity is a life-annuity which is to increaſe at the rate of £. 1 every year during the whole poſſible continuance of a life whoſe complement is forty-one years (or whoſe age is about forty-five) the preſent value of it will be, by the laſt theorem, £. 133 14*s.*; taking the probability of the duration of human life according to Mr. DE MOIVRE's hypotheſis; which agrees nearly with Dr. HALLEY's Table of Obſervations.

VII. *An Account of the* Romanſh *Language. By* Joſeph Planta, *F. R. S. In a Letter to Sir* John Pringle, *Bart. P. R. S.*

R. Nov. 10, 1775.

SIR,

British Muſeum, June 10, 1775.

THE bible lately preſented to the Royal Society by the Count DE SALIS, being a verſion into a language as little attended to in this country, as it may appear curious to thoſe who take pleaſure in philological inquiries; I embrace this opportunity to communicate to you, and, with your approbation, to the Society, all that I have been able to collect concerning its hiſtory and preſent ſtate.

This language is called *Romanſh*, and is now ſpoken in the moſt mountainous parts of the country of the Griſons, near the ſources of the Rhine and the En. It conſiſts of two main dialects; which, though partaking both of the above general name, differ however ſo widely as to conſtitute in a manner two diſtinct languages. Books are printed in both of them; and each, though it be univerſally underſtood in its reſpective diſtrict, is yet ſub-divided into almoſt as many ſecondary dialects as there are villages in which it is ſpoken; which differ, however, but little except in the pronunciation. One of the main dialects, which is ſpoken in the Engadine, a valley extending from the

the ſource of the En to the frontiers of the Tyroleſe, is by the inhabitants called *Ladin.* It admits of ſome variation, even in the books, according as they are printed either in the upper or the lower part of this province. The abovementioned bible is in the dialect of the lower Engadine; which, however, is perfectly underſtood in the upper part of that province, where they uſe no other verſion. The other dialect, which is the language of the Grey, or Upper League, is diſtinguiſhed from the former by the name of *Cialover*: and I muſt here obſerve, that in the very center, and moſt inacceſſible parts of this latter diſtrict, there are ſome villages ſituated in narrow vallies, called Rheinwald, Cepina(a), &c. in which a third language is ſpoken, more ſimilar to the German than to either of the above idioms, although they be neither contiguous, nor have any great intercourſe with the parts where the German is uſed.

It being impoſſible to form any idea of the origin and progreſs of a language, without attending to the revolutions that may have contributed to its formation and ſubſequent variations; and this being particularly the caſe in the preſent inſtance, wherein no ſeries of documents is extant to guide us in our reſearches; I ſhall briefly recapitulate the principal events which may have affected the language of the Griſons, as I find them related by authors of approved veracity(b).

AMBI-

(a) TSCHUDI Rhæt. Deſcrip. p. 43. MERIAN Topogr. Helvet. p. 64.

(b) SPRECHER, SIMLER, TSCHUDI, SCHEUCHZER. CAMPELL'S Chronicle is looked upon as the moſt authentic and circumſtantial; but there being only

a few

AMBIGATUS, the firft king of the Celtic Gaul upon record, who (c) about 400 (d) years before CHRIST, governed all the country fituated between the Alps and the Pyrenæan mountains, fent out two formidable armies under the command of two of his nephews; one of whom, named SEGOVISIUS, forced his way into the heart of Germany: and the other, BELLOVISIUS, having paffed the Alps, penetrated into Italy as far as the fettlements of the Tufcans, which at that time extended over the greateft part of the country now called Lombardy. Thefe, and feveral other fwarms of invaders whom the fucceffes of the former foon after attracted, having totally fubdued that country, built Milan, Verona, Brefcia, and feveral other confiderable towns, and governed with fuch tyrannic fway, efpecially over the nobility, whofe riches they coveted and fought by every means to extort from them, that moft of the principal families, joining under the conduct of RHÆTUS (e), one of the moft diftinguifhed perfonages among them, retired with the beft part of their effects and attendants among the fteepeft mountains of the Alps, near the fources of the Rhine, into the diftrict which is now called the Grey League.

a few manufcript copies of it extant in the hands of private perfons in the Grifons, I have not been able to avail myfelf of his refearches. GULER and STUMPFIUS might alfo have furnifhed fome material information; but neither of them have I had an opportunity of infpecting.

(c) LIV. lib. v. c. 34.

(d) Other authors place the reign of this king 180 years earlier.

(e) PLIN. lib. iii. c. 5. JUSTIN. lib. xx. c. 5.

The motive of their flight, their civil deportment, and perhaps more ſo, the wealth they brought with them, procured them a favourable reception from the original inhabitants of that inhoſpitable region, who are mentioned by authors (f) as being a Celtic nation, fabulouſly conjectured from their name (Λειποντιοι, *relicti)* to have been left there by HERCULES in his expedition into Spain.

The new adventurers had no ſooner climbed over the higheſt precipices, but thinking themſelves ſecure from the purſuits of their rapacious enemies, they fixed in a valley which, from its great fertility in compariſon of the country they had juſt paſſed, they called Domeſtica (g). They intermixed with the old inhabitants, and built ſome towns and many caſtles, whoſe preſent names manifeſtly beſpeak their origin (h). They ſoon after ſpread all over the country, which took the name of Rhætia from that of their leader; and introduced a form of government ſimilar to their own, of which there are evident traces at this day, eſpecially in the adminiſtration of juſtice; in which a *Laertes*, or Preſident, now called Landamman, or Miniſtral, together with twelve *Lucumones* (i), or Jurors determine, all cauſes, both civil and criminal (k): and

(f) CLUVER, Ital. Antiq. lib. i. c. 14.

(g) Probably by them pronounced *Tomiliaſca*, the name it now bears.

(h) *Tuſis* (Tuſcia) and in Italian *Toſana*, the principal place; *Rhealta* (Rhetia alta); *Rheambs* (Rhetia ampla); *Rhazunz* (Rhetia ima), and above twelve other caſtles, the remains of which are now to be ſeen in the valley *Tomiliaſca*.

(i) In ſome communities there are fourteen jurors beſides the Landamman.

(k) SERV. in Æneid, lib. viii. 65. lib. x. 202. SPRECH. Pall. Rhæt. p. 9. SIML. Rep. Helv. p. 281. ed. 1735.

LIVY[(l)], although he erroneouſly pretends that they retained none of their ancient cuſtoms, yet allows that they continued the uſe of their language, though ſomewhat adulterated by a mixture with that of the Aborigines.

I muſt here interrupt the thread of this narration by obſerving, that the only way to account for the preſent uſe of a different language in the center and moſt craggy parts of the Grey League, is by allowing that the Tuſcans, who, from the delicacy of their conſtitutions and habits, were little able, and leſs inclined, to encounter the hardſhips of ſo ſevere a climate and ſo barren a ſoil, never attempted to mix with the original and more ſturdy inhabitants of that unfavoured ſpot; but left them and their language, which could only be a Celtic idiom, in the primitive ſtate in which they found them[(m)].

But to proceed;—ſeveral Roman families, dreading the fury of the Carthaginians under HANNIBAL; and perhaps, ſince during the rage of the civil wars, and the ſubſequent oppreſſive reigns, interior commotions and foreign invaſions, forſook the Latium and Campania, and reſorted for a peaceful enjoyment of their liberty, ſome into the iſlands where Venice now ſtands; and many into the mountains of the Griſons, where they chiefly fixed their reſidence in the Engadine[(n)], as appears not only from the teſtimonies of authors[(o)], but alſo

(*l*) LIV. lib. v. c. 33.

(*m*) SPRECH. p. 214. MER. l. c.

(*n*) *En Code Ino*, perhaps the vulgar Roman phraſe expreſſing *In Capite Oeni*. There are other etymologies, but all equally uncertain.

(*o*) SPRECH. p. 10.

from

from the names of ſeveral places and families which are evidently of Roman derivation (p).

The inhabitants theſe emigrants found in that place of refuge could not but be a mixture of the Tuſcans and original Lepontii: and the two languages which met upon this occaſion muſt, at the very firſt, have had ſome affinity; as the Tuſcan, which derived immediately from the Greek, is known to have had a great ſhare in the formation of the Roman. But as it is generally obſerved, that the more poliſhed people introduce their native tongue wherever they go to reſide in any conſiderable numbers, the arrival of theſe ſucceſſive colonies muſt gradually have produced a conſiderable change in the language of the country in which they ſettled (q); and this change gave riſe to the dialect ſince called Ladin, probably from the name of the mother country of its principal authors (r).

Although the name of *Romanſh*, which the whole language bears, ſeem to be a badge of Roman ſervitude, yet the conqueſt of that nation, if ever effected, could not have produced a great alteration in a language which muſt already have been ſo ſimilar to their own; and its general name may as well be attributed to the pacific as

(p) *Lavin* (Lavinium), *Sus* (Suſa), *Zernetz* (Cerneto), *Ardetz* (Ardea), &c.

(q) SPRECH. p. 10.

(r) A parallel inſtance of the formation of a language by Roman colonies is the idiom of Moldavia; which, according to Prince CANTEMIR'S account of that country, has ſtill many traces of its Latin origin, and which, though engrafted upon the Dacian, and ſince upon the Sclavonian dialects of the Celtic, may ſtill be conſidered as a ſiſter language to that I am here treating of.

to the hoſtile Romans. But when we conſider that a coalition of the two main dialects, which differ ſo far as not to be reciprocally underſtood, muſt have been the inevitable conſequence of a total reduction; and that ſuch a coalition is known never to have taken place, we may lay the greater ſtreſs upon the many paſſages of ancient authors (s), in which it is implied that the boaſted victories of the Romans over the Rhæti, for which public honours had been decreed to L. MUNATUS, M. ANTHONY, DRUSUS, and AUGUSTUS, amounted to no more than frequent repulſes of thoſe hardy people into their mountains; out of which their want of ſufficient room and ſuſtenance, (which in our days drives conſiderable numbers of them into the ſervices of foreign powers) compelled them at times to make deſperate excurſions in queſt of neceſſaries. And we may alſo from theſe collected authorities be induced to give the greater credit to the commentator of LUCAN (t), and to the modern hiſtorians (u), who poſitively aſſert, that the people living near the ſources of the Rhine and the En were never totally ſubdued by the

(s) Videre Rhæti bella *ſub* Alpibus
Druſum gerentem et Vindelici. HOR. lib. 4. Od. iv.
——————— immaneſque Rhætos
Auſpiciis *repulit* ſecundis. Ibid. Od. xiv.
Fundat ab extremo flavos aquilone Süevos
Albis, et *indomitum Rheni Caput.* LUC. lib. ii. 52.
————— Rhenumque minacem
Cornibus infractis. CLAUD. Laud. Stilich. lib. i. 220.

(t) HORTEN. in LUCAN, p. 163. edit. 1578. fol.

(u) SPRECH. p. 18, &c.

Roman arms; but only repelled in their attempts to harrafs their neighbours.

This whole country, however, from its central fituation, could not but be annumerated to one of the provinces of the empire; and accordingly we find that Rhætia itfelf (which by the accounts of ancient geographers (x) appears to have extended its limits beyond the lake of Conftance, Augfburg, and Trent, towards Germany, and to Como and Verona towards Italy) was formed into a Roman province, governed by a pro-conful or procurator, who refided at Augfburg; and that when in the year 119, the Emperor ADRIAN divided it into Rhætia *prima* and *fecunda*, the governor of the former, in which the country I am now fpeaking of muft have been comprized, took up his refidence in two caftles fituated where Coire now ftands, whilft the other continued his feat at Augfburg. But notwithftanding thefe appearances, no trace or monument of Roman fervitude is to be met with in this diftrict, except the ambiguous name of one mountain (y), fituated on the fkirts of thefe highlands, and generally thought to have been the *non plus ultra* of the Roman arms on the Italian fide.

From the difficulty thofe perfevering veterans experienced in keeping this ftubborn people in awe, I mean to infer that fuch ftrenuous affertors of their independence, whom the flattering pens of OVID and HORACE reprefent as formidable even to AUGUSTUS, and prefer-

(x) STRABO, lib. IV. fub fin. CLUVER, Ital. vet. lib. I. c. 16.

(y) *Julius Mons*, SCHEUCHZER Iter. Alp. p. 114.

ring

ring death to the loss of their liberties (z), favoured by the natural strength and indigence of their country, were not very likely to be so far subdued by any foreig · ower inferior to the Roman, as to suffer any considerable revolution in their customs and language: for as to the irruptions of the Goths, Vandals, and Lombards, in the fifth and sixth centuries, besides a profound silence in history concerning any successful attempt of those barbarians upon this spot, it is scarce credible, that any of them should have either wished or endeavoured to settle in a country, perhaps far less hospitable than that they had just forsaken, especially after they had opened to themselves a way into the fertile plains of Lombardy.

Some stress must be laid upon this inference, as the history of what befel this country after the decline of the Roman empire is so intimately blended with that of Suabia, the Tyrolese, and the lower parts of the Grisons, which are known to have fallen to the share of the rising power of the Franks, that nothing positive can be drawn from authors as to the interior state of this small tract. The victory gained in the year 496 near Cologn, by CLOVIS I. king of the Franks, over the Almains, who had wrested from the Romans all their dominions on the Northern side of the Alps; and the defeat of both Romans and Goths in Italy, in the year 549, by the treacherous arms of THEODEBERT king of Austrasia, whose do-

(z) Rhætica nunc præbent Thraciaque arma metum.

OVID. Trist. lib. ii. 226.

Devota morti pectora liberæ. HOR. 4 lib. Od. xiv.

minions ſoon after devolved to the crown of France, neceſſarily gave the aſpiring Merovingian race a great aſcendency over all the countries ſurrounding the Griſons; and accordingly we find, that this diſtrict alſo was ſoon after, without any military effort, conſidered as part of the dominions of the reviving Weſtern empire. But it does not appear that thoſe monarchs ever made any other uſe of their ſupremacy in theſe parts than, agreeable to the feudal ſyſtem which they introduced, to conſtitute dukes, earls, preſidents and bailiffs, over Rhætia; to grant out tenures upon the uſual feudal terms; and conſequently to levy forces in moſt of their military expeditions.

It muſt, however, be obſerved, that theſe feudal ſubſtitutes were ſeldom, if ever, ſtrangers: thoſe who are upon record to the latter end of the eighth century having all been choſen from among the nobility of the country (a). And that no foreign garriſons were ever maintained for any continuance of time in theſe parts, appears from a circumſtance related by their annaliſts (b); who ſay, that an inroad of the Huns in 670, when external forces would probably have been very acceptable to the natives, was repulſed merely by a concourſe of the inhabitants.

Hiſtory continues to furniſh us with proofs of the little connexion this people had with other nations in their domeſtic affairs, notwithſtanding their dependance upon a foreign power. In the year 780, the Biſhop of Coire, who by the conſtitution of that ſee can only be a na-

(*a*) SPRECH. p. 52—55. (*b*) Ibid. p. 58.

5 tive,

tive (c), obtained from CHARLEMAIN, beſides many conſiderable honours and privileges in the empire, a grant of the ſupreme authority in this country, by the inveſtiture of the office of hereditary preſident or bailiff over all Rhætia. His ſucceſſors not only enjoyed this prerogative to the extinction of the Carlovingian race of emperors in 911, but received accumulated favours from other ſucceeding monarchs, as the bigoted devotion of thoſe times or motives of intereſt prompted them. And ſo far did their munificence gradually extend, that the ſole property of one of the three leagues (d) was at one time veſted in the hands of the biſhop.

This prelate and the nobles, the greateſt part of whom became his retainers, availed themſelves, like all the German princes, of the confuſion, diviſions, and interreigns which frequently diſtracted the empire in the ſucceeding centuries, in order to eſtabliſh a firm and unlimited authority of their own. Henceforth the annals of this country furniſh us with little more than catalogues of the biſhops and dukes, who were ſtill, at times, nominated by the emperors; and of the domains granted out by them to different indigenate families; with accounts of the atrocious cruelties exerciſed by theſe lords over their vaſſals; and with anecdotes of the proweſs of the

(c) This privilege hath at times been waved; but never without ſome plauſible pretence, and a formal reſcript, acknowledging the excluſive right.

(d) The League *Cadéa*, or of the *Houſe of God*, ſo called from the cathedral of the biſhopric of Coire, which is ſituated in its capital.

natives in ſeveral expeditions into Italy and Paleſtine, in which they ſtill voluntarily accompanied the emperors.

The repeated acts of tyranny exercifed by thoſe arbitrary deſpots, who had new ſhaken off all manner of reſtraint, at length exaſperated the people into a general revolt, and brought on the confederacy; in which the biſhop and moſt of the nobles were glad to join, in order to ſcreen themſelves from the fury of the inſurgents.

The firſt ſtep towards this happy revolution was made by ſome *venerable old men dreſſed in the coarſe grey cloth* of the country, who in the year 1424 met privately in a wood near a place called Truns, in the Upper League; where, *impreſſed with a ſenſe of their former liberties*(e), they determined to remonſtrate againſt, and oppoſe, the violent proceedings of their oppreſſors. The abbot of Diſſentis was the firſt who countenanced their meaſures; their joint influence gradually prevailed over ſeveral of the moſt moderate among the nobles; and hence aroſe the League which, from the colour of its firſt promoters, was ever after called the Grey League; which, from its being the firſt in the bold attempt to ſhake off the yoke of wanton tyranny, hath ever ſince retained the pre-eminence in rank before the two other leagues; and which hath even given its name to the whole country, whoſe inhabitants, from the circumſtances of their deliverance, pride themſelves in the appellation of *Griſons*,

(e) Canitie griſeoque amictu venerandi.—Memores adhuc antiquæ libertatis. SPRECH. p. 189.

or

or the *grey-ones* (f). From this period nothing hath ever affected their freedom and abſolute independence; which they now enjoy in the moſt unlimited ſenſe, in ſpite of the repeated efforts of the houſe of Auſtria to recover ſome degree of aſcendency over them.

From this conciſe view of the hiſtory of the Griſons, in which I have carefully guarded againſt favouring any particular hypotheſis, it appears, that as no foreign nation ever gained any permanent footing in the moſt mountainous parts of this country ſince the eſtabliſhment of the Tuſcans and Romans, the language now ſpoken could never have ſuffered any conſiderable alteration from extraneous mixtures of modern languages. And to thoſe who may object, that languages like all other human inſtitutions will, though left to themſelves, be inevitably affected by the common revolutions of time, I ſhall obſerve, that a language, in which no books are written, but which is only ſpoken by a people chiefly devoted to arms and agriculture, and conſequently not cultivated by the criticiſms of men of taſte and learning, is by no means expoſed to the viciſſitudes of thoſe that are poliſhed by refined nations (g); and that, however paradoxical it may appear, it is nevertheleſs true, that the degeneracy of a language is more frequently to be attributed to an extravagant refinement than to the neglect of an illiterate

(f) The following barbarous diſtich is ſometimes inſcribed to the arms of the three Leagues,

> Fœdera ſunt cana, cana fidès, cana libertas:
> Hæc tria ſub uno continentur corpore Rhæto.

(g) See Dr. PERCY's Preface to his Tranſlation of MALLET's Northern Antiquities, p. xxii. where this queſtion is more amply diſcuſſed.

illiterate people, unlefs indeed external caufes interfere. May we not hence conclude, that as the Romanfh has never been ufed in any regular compofition in writing till the fixteenth century, nor affected by any foreign invafion or intimate connexion, it is not likely to have received any material change before the period of its being written? And we have the authority of the books fince printed to prove, that it is at prefent the identical language that was fpoken two hundred years ago. Thefe arguments will receive additional weight from the proofs I fhall hereafter give of the great affinity there is between the language as it is now fpoken, and the Romance that was ufed in France nine centuries ago.

When we further confider the facts I have above briefly related, the wonder will ceafe, that in a clufter of mountains, fituated in the center of Europe, a diftinct language (not a dialect or jargon of thofe fpoken by the contiguous nations, as hath been generally imagined) fhould have maintained itfelf through a feries of ages, in fpite of the many revolutions which frequently changed the whole face of the adjacent countries. And indeed, fo obftinately tenacious are thefe people of their independency, laws, cuftoms, and confequently of their very language, that, as hath been already obferved, their form of government, efpecially in judicial matters, ftill bears evident marks of the ancient Tufcan conftitution; and that, although they be frequently expofed to inconveniences from their ftubbornnefs in this refpect, they have not yet been prevailed upon to adopt the Gregorian reformation of the calendar.

As

As to the nature of this language, it may now be advanced, with ſome degree of confidence, that the *Cialover* owes its origin to a mixture of the Tuſcan and of the dialect of the Celtic ſpoken by the Lepontii; and that the introduction of the vulgar Roman affected it in ſome degree, but particularly gave riſe to the *Ladin*; the vocabulary of which, as any one may be convinced by inſpecting a few lines of the bible, hath a great affinity with that of the Latin tongue. But theſe aſſertions reſt merely upon hiſtorical evidence; for as to the *Cialover*, all that it may have retained of the Tuſcan or Roman, is ſo much disfigured by an uncouth pronunciation and a vague orthography, that all etymological enquiries are thereby rendered intricate and unſatisfactory. And as to the *Ladin*, although its derivation be more manifeſt, yet we are equally at a loſs from what period or branch of the Latin tongue to trace its real origin; for I have found, after many tedious experiments, that even the vocabulary, in which the reſemblance is moſt evident, differs equally from the claſſical purity of TULLY, CÆSAR, and SALLUST, as it does from the primitive Latin of the twelve tables, of ENNIUS, and the *columna roſtralis* of DUILLIUS, which hath generally been thought the parent of the Gallic Romance; as alſo from the trivial language of VARRO, VEGETIUS, and COLUMELLA. May we not from this circumſtance infer, that, as is the caſe in all vernacular tongues, the vulgar dialect of the Romans, the *ſermo uſualis, ruſticus, pedeſtris,* (b) of which

(b) Conf. Mem. des Inſcrip. tom. xxiv. p. 608.

there

there are no monuments extant, differed very widely both in pronunciation and conftruction from that which hath at any time been ufed either in writing or in the fenate?

The grammatical variations, the fyntax, and the genius of the language, muft in this, as well as in feveral other modern European tongues, have been derived from the Celtic; it being well known, that the frequent ufe of articles, the diftinction of cafes by prepofitions, the application of two auxiliaries in the conjugations, do by no means agree with the Latin turn of expreffion; although a late French academician (i), who hath taken great pains to prove that the Gallic Romance was folely derived from the Roman, quotes feveral inftances in which even the moft claffical writers have in this refpect offended the purity of that refined language. It cannot here be denied, that as new ideas always require new figns to exprefs them, fome foreign words, and perhaps phrafes, muft neceffarily, from time to time, have infinuated themfelves into the Romanfh by the military and fome commercial intercourfe of the Grifons with other nations; and this accounts for feveral modern German words which are now incorporated into the language of the Engadine (k).

The little connexion there is in mountainous countries between the inhabitants of the different vallies, and the abfolute independence of each jurifdiction in this

(i) BONAMY, v. Mem. des Infcrip. l. c.

(k) *Tapferdà*, Tapferkeit, Bravery; *Nardà*, Narheit, Folly; *Elinot*, Kleinod, a Jewel; *Graf*, Graf, a Count; *Baur*, Baur, a Peafant, &c.

diftrict,

diſtrict, which ſtill leſſens the frequency of their intercourſe, alſo accounts, in a great meaſure, for the variety of ſecondary dialects ſubſiſting in almoſt every different community or even village.

The oldeſt ſpecimens of writing in this language are ſome dramatical performances in verſe upon ſcriptural ſubjects; which are extant only in manuſcript. The hiſtories of SUSANNA, of the Prodigal Son, of JUDITH and HOLOFERNES, and of ESTHER, are among the firſt; and are ſaid to have been compoſed about the year 1560. The books that have ſince been printed are chiefly upon religious ſubjects; and among thoſe that are not ſo, the only I have ever heard of are a ſmall code of the laws of the country in the Cialover dialect, and an epitome of SPRECHER's Chronicle, by DA PORTA, in the Ladin.

The language ſpoken in Gaul from the fifth to the twelfth centuries being evidently a mixture of the ſame Roman and Celtic ingredients, and partaking of the ſame name with thoſe of the Griſons; it will, I hope, not be thought foreign to the ſubject of this letter, if I enter into a few particulars concerning it, as it ſeems to have been an eſſential part, or rather the trunk, of the language, the hiſtory of which I am endeavouring to elucidate.

One of the many inſtances how little the laboured reſearches of philologiſts into the origin of languages are to be depended upon, is the variety of opinions entertained by French authors concerning the formation of the

the Gallic Romance. A learned Benedictine (l) firſt ſtarts the conjecture, and then maintains it againſt the attacks of an anonymous writer, that the vulgar Latin became the univerſal language of Gaul immediately after CÆSAR's conqueſt, and that its corruption, with very little mixture of the original language of the country, gradually produced the Romance towards the eighth century. BONAMY (m), on the other hand, is of opinion, that ſoon after that conqueſt, a corruption of vulgar Latin by the Celtic formed the Romance, which he takes to be the language always meant by authors when they ſpeak of the *Lingua Romana* uſed in Gaul. The author of the Celtic Dictionary (n) tells us, that the Romance is derived from the *Latin*, the *Celtic*, which he more frequently calls Gallic, and the *Teutonic*; in admitting of which latter he deviates from moſt other authors (o), who deny that the Teutonic had any ſhare in the compoſition of the Romance, ſince the Franks found it already eſtabliſhed when they entered Gaul, and were long before they could prevail upon their new ſubjects to adopt any part of their own mother tongue, which however appears to have been afterwards inſtrumental in the formation of the modern French. DUCLOS (p), guided, I imagine, by DU CANGE (q), whoſe opinion appears to be

(l) RIVET, Hiſt. Litt. de la France, tom. vii. p. 1. et ſeq.

(m) Mem. des Inſcrip. tom. xxiv. p. 594.

(n) BULLET, Mem. de la Langue Celtique, tom. i. p. 23.

(o) Mem. des Inſcrip. tom. xxiv. p. 603.

(p) Ibid. tom. xv. p. 575. et ſeq.

(q) Præf. Gloſſ. n. xiii.

the moſt ſober and beſt authenticated, maintains that the vulgar Latin was undoubtedly the foundation of the Romance; but that much of the Celtic gradually inſinuated itſelf in ſpite of the policy of the Romans, who never failed to uſe all their endeavours in order to eſtabliſh their language wherever they ſpread their arms.

Among this variety of conjectures and acute controverſies, I find it however agreed on all hands, that the vocabulary of the Roman, and the idiom of the Celtic have chiefly contributed to the formation of the Gallic Romance, which is ſufficient to prove that it partakes of a common origin with that of the Griſons.

There are inconteſtable proofs that this language was once univerſal all over France; and that this, and not immediately the Latin, hath been the parent of the Provençal, and afterwards of the modern French, the Italian, and the Spaniſh. The oath taken by LEWIS the Germanic, in the year 842, in confirmation of an alliance between him and CHARLES the Bald, his brother, is a deciſive proof of the general uſe of the Romance by the whole French nation at that time, and of their little knowledge of the Teutonic, which being the native tongue of LEWIS, would certainly have been uſed by him in this oath, had it been underſtood by the French to whom he addreſſed himſelf. But NITHARDUS(r), a cotemporary writer and near relation to the contracting parties, informs us, that LEWIS took the oath in the Romance language, in order that it might be underſtood by

(r) DU CHESNE, Hiſt. Franc. tom. ii. p. 374.

the French nobility who were the ſubjects of CHARLES; and that they, in their turn, entered into reciprocal engagements in *their own language*, which the ſame author again declares to have been the Romance, and not the Teutonic; although one would imagine that, had they at all underſtood this latter tongue, they could not but have uſed it upon this occaſion, in return for the condeſcenſion of LEWIS.

As a compariſon between this language and the Romanſh of the Griſons cannot be conſidered as a mere object of curioſity; but may alſo ſerve to corroborate the proofs I have above alledged of the antiquity of the latter, I have annexed in the appendix(s), a tranſlation of this oath into the language of Engadine, which approaches neareſt to it; although I muſt obſerve, that there are in the other dialect ſome words which have a ſtill greater affinity with the language of the oath, as appears by another tranſlation I have procured, in which both dialects are indifferently uſed. To prevent any doubts concerning the veracity of theſe tranſlations, I muſt here declare, that I am indebted for them, and for ſeveral anecdotes concerning that language, to a man of letters, who is a native and hath long been an inhabitant of the Griſons, and is lately come to reſide in London. I have added to this comparative view of thoſe two languages the Latin words from which both ſeem to have been derived; and, as a proof of the exiſtence of the Gallic Romance in France down to the twelfth century, I have

(s) N° I.

alſo

alſo ſubjoined the words uſed in that kingdom at that period, as they are given us by the author of the article *(Langue) Romane*, in the French Encyclopedie.

To the compariſon of the two Romances, and the ſimilarity of their origin, I may now with confidence add the authority of FONTANINI[t] to prove, that they are one and the ſame language. This author, ſpeaking of the ancient Gallic Romance, aſſerts that it is now ſpoken in the country of the Griſons; though, not attending to the variety of dialects, ſome of which have certainly nothing of the Italian, he ſuppoſes it to have been altogether adulterated by a mixture of that modern tongue.

Whilſt the Griſons neglected to improve their language, and rejected, or indeed were out of the reach of every refinement it might have derived from poliſhed ſtrangers, the taſte and fertile genius of the Troubadours, foſtered by the countenance and elegance of the brilliant courts and ſplendid nobility of Provence, did not long leave theirs in the rough ſtate in which we find it in the ninth century. But the change having been gradual and almoſt imperceptible, the French hiſtorians have fixed no *epocha* for the tranſition of the Romance into the Provençal. That the former language had not received any conſiderable alteration in the twelfth century may be gathered from the compariſon in the appendix: and that it ſtill bore the ſame name, appears from the titles of ſeveral books which are ſaid to have been written in, or tranſlated into, the Romance. But though mention is made

(t) Eloq. Ital. p. 44.

of that name even after this æra, yet upon examining impartially what is given us for that language in this period, it will be found ſo different from the Romance of the ninth century, that to trace it any further would be both a vain and an extravagant purſuit.

Admitting, however, the univerſal uſe of the Romance all over France down to the twelfth century, which no French author hath yet doubted or denied; and allowing that what the writers of thoſe times ſay of the Gallic is to be underſtood of the Romance, as appears from chronological proofs, and the expreſſions of ſeveral authors prior to the fifth century (u); who, by diſtinguiſhing the *Gallic* both from the *Latin* and the *Celtic*, plainly indicate that they thereby mean the Romance, thoſe being the only three languages which, before the invaſion of the Franks, could poſſibly have been ſpoken, or even underſtood in Gaul: admitting theſe premiſes, I ſay, it neceſſarily follows, that the language introduced into England under ALFRED, and afterwards more univerſally eſtabliſhed by EDWARD the Confeſſor, and WILLIAM the Conqueror, muſt have been an emanation of the Romance, very near akin to that of the abovementioned oath, and conſequently to that which is now ſpoken in the Alps.

The intercourſe between Britain and Gaul is known to have been of a very early date; for even in the firſt

(u) Fidei commiſſa quocunque Sermone relinqui poſſunt, non ſolum *Latino* vel Græco, ſed etiam Punico vel *Gallicano*. Digeſt. l. xxii. tit. 1. § 11.

Tu autem vel *Celtice*, vel ſi mavis *Gallice*, loquere. SULP. SEV. Dial. i. § 6. ſub fin.

century

century we find, that the Britiſh lawyers derived the greateſt part of their knowledge from thoſe of the continent (x); whilſt, on the other hand, the Gallic Druids are known to have reſorted to Britain for inſtruction in their myſterious rites. The Britons, therefore, could not be totally ignorant of the Gallic language. And hence it will appear, that GRIMBALD, JOHN, and the other doctors, introduced by ALFRED (y), could find no great difficulty in propagating their native tongue in this iſland; which tongue, at that interval of time, could only be the true Romance, ſince they were cotemporaries with LEWIS the Germanic.

That the Romance was almoſt univerſally underſtood in this kingdom under EDWARD the Confeſſor, it being not only uſed at court, but frequently at the bar, and even ſometimes in the pulpit, is a fact too well-known and atteſted (z) to need my further authenticating it with ſuperfluous arguments and teſtimonies.

DUCLOS, in his Hiſtory of the Gallic Romance (a), gives the abovementioned oath of LEWIS as the firſt monument of that language. The ſecond he mentions is the code of laws of WILLIAM the Conqueror (b), whom the leaſt proficient in the Engliſh hiſtory knows to have rendered his language almoſt univerſal in this kingdom.

(x) Gallia Cauſidicos docuit facunda Britannos. JUV. Sat. XV III.
(y) WILLIAM of Malmsb. l. ii. c. 4.
(z) INGULPH. paſſim. DU CHESNE, tom. iii.
(a) Mem. des Inſcrip. tom. xvii. p. 179.
(b) WILKINS, Leges Anglo-Sax.

How

How little progreſs it had yet made towards the modern French; and how great an affinity it ſtill bore with the preſent Romanſh of the Griſons, will appear from the annexed tranſlation of the firſt paragraph of theſe laws into the latter tongue (c).

If we may credit DU CANGE (d), who grounds his aſſertion upon various inſtruments of the kings of Scotland during the twelfth century, the Romance had alſo penetrated into that kingdom before that period.

The ſame corruption, or coaleſcence, which gave riſe to the Gallic Romance, and to that of the Griſons, muſt alſo have produced in Italy a language, if not perfectly ſimilar, at leaſt greatly approaching to thoſe two idioms. Nor did it want its Northern nations to contribute what the two other branches derived from that ſource (e). But be the origin what it will, certain it is, that a jargon very different from either the Latin or the Italian was ſpoken in Italy from the time of the irruptions of the barbarians to the ſucceſsful labours of DANTE and PETRARCA; that this jargon was uſually called the *vulgar idiom*; but that SPERONI (f), the father of Italian literature, and others, frequently call it the *common Italian Romance*. And if FONTANINI'S (g) authorities be ſufficient, it appears that even the Gallic Romance, by the reſidence of the Papal Court

(c) Append. N° II.

(d) Præf. Gloſſ. n. xxi.

(e) FONTANINI, p. 4.

(f) SPERON. Dial. paſſim.—Conf. MENAGE, Orig. della Ling. Ital. voce ROMANZA.

(g) FONT. p. 17.

at

at AVIGNON, and from other causes, made its way into Italy before it was polished into the Provençal.

As to Naples and Sicily, the expulsion of the Saracens by the Normans, under ROBERT GUISCARD in 1059, must have produced in that country nearly the same effect, a similar event soon after brought about in England. And in fact we have the authority of WILLIAM of Apulia(*h*) to prove, that the conquerors used all their efforts to propagate their language and manners among the natives, that they might ever after be considered only as one people. And HUGO FALCLAND(*i*) relates, that in the year 1150, Count HENRY refused to take upon him the management of public affairs, under pretence of not knowing the language of the French; which, he adds, was absolutely necessary at court.

That the language of the Romans penetrated very early into Spain, appears most evidently from a passage in STRABO(*k*), who asserts, that the Turditani inhabiting the banks of the Boetis, now the Guadalquivir, forgot their original tongue, and adopted that of the conquerors. That the Romance was used there in the fourteenth century appears from a correspondence between ST. VINCENT of Ferrieres and Don MARTIN, son of PETER the IVth of Arragon(*l*); and that this language must once have been common in that kingdom appears manifestly from the present name of the Spanish, which

(*h*) MURAT. Script. Ital. tom. v. p. 255. (*i*) Ibid. tom. vii. p. 322.
(*k*) Lib. iii. (*l*) MABILL. an. l. 64. n. 124.

is ſtill uſually called Romance(m). Theſe circumſtances conſidered, I am not ſo much inclined to diſcredit a fact related by MABILLON(n), who ſays, that in the eighth century a paralytic Spaniard, on paying his devotions at the tomb of a ſaint in the church of Fulda, converſed with a monk of that abbey, who, *becauſe he was an Italian*, underſtood the language of the Spaniard. Neither does an oral tradition I heard ſome time ago appear now ſo abſurd to me, as it did when it was firſt related to me, which ſays, that two Catalonians travelling over the Alps, were not a little ſurprized when they came into the Griſons, to find that their native tongue was underſtood by the inhabitants, and that they could comprehend moſt of the language of that country.

The univerſality of the Romance in the French dominions during the eleventh century, alſo accounts for its introduction in Paleſtine and many other parts of the Levant by GODFREY DE BOUILLON, and the multitude of adventurers who engaged under him in the Cruſade. The aſſizes or laws of Jeruſalem, and thoſe of Cyprus, are ſtanding monuments of the footing that language had obtained in thoſe parts; and if we may truſt a Spaniſh hiſtorian of ſome reputation (o) who reſided in Greece in the thirteenth century, the Athenians and the inhabitants of Morea ſpoke at that time the ſame language that was uſed in France. And there is great reaſon to ima-

(m) OROZCO, Teſ. Caſtill. voce ROMANCE—Conf. CRESCIMB. Volg. Poeſ. l. v. c. 1. (n) Act. Ben. Sæc. 3. p. 2. p. 258.

(o) RAYM. MONTANERO Chronica de JUAN I.

gine,

gine, that the affinity the *Lingua Franca* bears to the French and Italian is intirely to be derived from the Romance, which was once commonly uſed in the ports of the Levant. The heroic atchievements and gallantry of the Knights of the Croſs alſo gave riſe to the ſwarm of fabulous narratives; which, though not an invention of thoſe days, were yet, from the name of the language in which they were written, ever after diſtinguiſhed by the appellation of *Romances* (p).

I ſhall now conclude this letter by obſerving, that far from preſuming that the Romance hath been preſerved ſo near its primitive ſtate only in the country of the Griſons, there is great reaſon to ſuppoſe that it ſtill exiſts in ſeveral other remote and unfrequented parts. When FONTANINI informs us (q) that the ancient Romance is now ſpoken in the country of the Griſons, he adds, that it is alſo the common dialect of the Friuleſe, and of ſome diſtricts in Savoy bordering upon the Dauphiné. And RIVET (r) ſeriouſly undertakes to prove, that the Patois of ſeveral parts of the Limouſin, Quercy, and Auvergne (which in fact agrees ſingularly with the *Romanſh* of the Griſons) is the very Romance of eight centuries ago. Neither do I doubt but what ſome inquiſitive traveller might ſtill meet with manifeſt traces of it in many parts of the Pyrenæans and other mountainous regions of Spain, where the Moors and other invaders have never penetrated. I have the honour to be, &c.

(p) HUET, Orig. des Rom. p. 126. ed. 1678.

(q) P. 43, 44.

(r) Hiſt. Litt. de la Fr. tom. vii p. 22.

A P P E N D I X.

N° I. Oath of LEWIS the Germanic.

1. Latin from which the Romances are derived. 2. Gallic Romance in which the oath was taken. 3 French of the twelfth century. 4. Romanſh of Engadine, called Ladin. 5. Romanſh of both dialects.

1. Pro Dei amore, et pro Chriſtiano populo, et noſtro
2. *Pro Deu amur, et pro Chriſtian poblo, et noſtro*
3. Por Deu amor, et por Chriſtian pople, et noſtre
4. *Per amur da Dieu, et per il Chriſtian poevel, et noſs*
5. Pro l'amur da Deus, et pro il Chriſtian pobel, et noſt

1. communi ſalvamento, de iſta die in abante, in quan-
2. *commun ſalvament, d'iſt di en avant, in quant*
3. commun ſalvament, de ſte di en avant, en quant
4. *commun ſalvament, da quiſt di in avànt, in quant*
5. commun ſalvament, d'iſt di en avant, in quant

1. tum Deus ſapere et poſſe mihi donat, ſic ſalvabo ego
2. *Deus ſavir et podir me dunat, ſi ſalvarai io*
3. Deu ſaveir et po.r me donne, ſi ſalvarai je
4. *Dieu ſavair et podair m'duna, ſhi ſalvaro ei*
5. Deus ſavir et podir m'dunat, ſhi ſalvaro io

1. ecciſtum meum fratrem KARLUM, et in adjutum ero
2. *ciſt meon fradre* KARLO, *et in adjudab er*
3. ciſt mon frere KARLE, et en adjude ſerai
4. *quiſt mieu fræer* CARLO, *et in adgiud li ſaro*
5. quiſt meu frad'r CARL, et in adjudh ſaro

1. in quaque una causa, sic quomodo homo per directum
2. *in cadhuna cosa, si cum om per dreit*
3. en cascune cose, si cum om per dreict
4. *in chiaduna chiossa, shi scho l'hom per drett*
5. in caduna cosa, si com om per drett

1. suum fratrem salvare debet, in hoc quod ille mihi
2. *son fardre salvar dist, in o quid il me*
3. son frere salver dist, en o qui il me
4. *sieu fraer salvar d'uess, in que chél a mi*
5. seu frad'r salvar dess, in que chél me

1. alterum sic faceret; et ab Lothario nullum placitum
2. *altresi fazet; et ab Ludher nul plaid*
3. altresi fascet; et a Lothaire nul plaid
4. *altresi fadschess; et da Lothar mai non prendrò io un*
5. altresi fazess; et da Lothar nul plaid mai

1. nunquam prehendam quod meo volle eccisti meo fratri
2. *nunquam prindrai qui meon vol cist meon fradre*
3. nonques prendrai qui par mon voil a cist mon frere
4. *plæd che con mieu volair a quist mieu fraer*
5. non prendrò che con meu voler a quist meu frad'r.

1. KARLO in damno sit.
2. KARLE *in damno sit.*
3. KARLE en dam seit.
4. CARLO *sai in damn.*
5. CARL in damn sia.

N° II.

N° II. The firſt Paragraph of the Laws of WILLIAM the Conqueror.

1. The Latin tranſlation. 2. The French original. 3. A tranſlation into the Romanſh of both dialects.

1. Hæ ſunt Leges et Conſuetudines quas WILLELMUS Rex
2. *Ce ſont les Leis et les Cuſtumes que li Reis* WILLIAM *grantut*
3. Que ſun las Leias e'ls Cuſtums que il Rei WILLELM ga-

1. conceſſit toto populo Angliæ poſt ſubactam terram.
2. *a tut le peuple de Engleterre aprés le conqueſt de la terre.*
3. rantit a tut il poevel d'Engelterra dapo il conquiſt della

1. Eædem ſunt quas EDWARDUS Rex Cognatus ejus obſer-
2. *Ice les meiſmes que le Reis* EDWARD *ſun Coſin tint*
3. terra. E ſun las medemas que il Rei EDWARD ſeu cuſrin

1. vavit ante eum. Scilicet: Pax Sanctæ Eccleſiæ,
2. *devant lui. Co eſt a ſaveir: Pais a Sainte Egliſe,*
3. tenet avant el. Co es da ſavir: Pæſh alla Sainta Ba-

1. cujuſcunque forisfacturæ quis reus ſit hoc tempore, et
2. *de quel forfait que home out fait en cel tens, et*
3. ſelga(a), da quel sfarfatt que om a fatt en quc temp, ct

1. venire poteſt ad ſanctam Eccleſiam, pacem habeat vitæ
2. *il pout venir a ſainte Egliſe, out pais de vie*
3. il pout venir alla Sainta Baſelga, haun pæſh da vitta

(a) The word *Eccleſia* being more modern in the Latin tongue than *Baſilica*, the Romanſh word *Baſelga* derived from the latter is an additional proof of the antiquity of this language.

et

1. et membri. Et ſi quis injecerit manum in eum qui
2. *et de membre. E ſe alquons meiſt main en celui qui*
3. et da members. E ſi alcun metta man a quel que l'a

1. matrem Eccleſiam quæſierit, ſive ſit Abbatia ſive
2. *la mere Egliſe requireit, ſe ceo fuſt u Abbeie u*
3. mamma Baſelga requira, qu'ella fuſs Abbatia u

1. Eccleſia religionis, reddat eum quem abſtulerit et
2. *Egliſe de religion, rendiſt ce que il javereit pris*
3. Baſelga da religiun, renda que qu'el ſavares prais, et

1. centum ſolidos nomine forisfacturæ, et matri Eccleſiæ
2. *e cent ſols de forfait, e de Mer Egliſe de*
3. cent ſolds da sfarfatt, et alla mamma Baſelga da

1. parochiali 20 ſolidos, et capellæ 10 ſolidos: Et qui fregerit
2. *paroiſſe* 20 *ſolds, e de Chapelle* 10 *ſolds: E que enfraiant*
3. parochia 20 ſolds, e da capella 10 ſolds: E que infrignand

1. pacem Regis in Merchenelega 100 ſolidis emendet;
2. *la pais le Rei en Merchenelae* 100 *ſolds les amendes;*
3. la pæſh del Rei in Merchenelae 100 ſolds d'amenda;

1. ſimiliter de compenſatione homicidii et de inſidiis
2. *altreſi de Heinfare e de aweit*
3. altreſi della compenſatiun del omicidii et inſidias

1. præcogitatis.
2. *purpenſed.*
3. perpenſadas.

VIII. *A*

VIII. *A Supplement to a Paper, entitled, Observations on the Population of* Manchester[(a)]. *By* Thomas Percival, *M. D. F. R. and A. S.*

TO THE REV. DR. HORSLEY.

REV. SIR,

R. Dec. 14, 1775.

A PAINFUL nervous complaint in my eyes, with which I have been troubled a few days, lays me under the necessity of writing to you by an *amanuensis*. I beg leave to return you my best thanks for your kind attention to my Memoir. If you think the following additions of importance, you are at liberty to annex them to it[(b)].

I have the honour to be, &c.

FROM the table of the number of males and females baptized in different places it appears[(c)], that the proportion of males to females baptized is nearly as 13 to 12, agreeable to the calculation of Mr. DERHAM; but the succeeding ones shew, the number of females alive considerably exceed the number of males, in a variety of places; and that the widows are almost double the number of widowers.

(a) See Philosoph. Transactions, vol. LXV. art. XXIII.

(b) These papers came too late to my hands to be inserted in the last publication. S. HORSLEY.

(c) See Philosoph. Transf. vol. LXV. p 333.

A com-

A comparative view of the numbers of males and females in different places.

Places.	Males.	Females.
Mancheſter,	10548	11933
Salford,	2248	2517
Townſhips of ditto,	947	958
Pariſh of Mancheſter,	6942	6844
Bolton,	2159	2392
Little Bolton,	361	410
Monton,	196	190
Hale,	140	136
Horwich,	149	136
Darwen,	900	950
Cockey,	320	391
Chowbent,	554	606
Ackworth,	340	388
Eaſtham,	451	461
Chinley,	181	168
Brownſide,	40	47
Bugsworth,	80	95
Aſhton under Line,	1406	1453
Pariſh of ditto,	2584	2513
Tattenhall pariſh,	382	399
Waverton pariſh,	310	332
Total,	31238	33339

A comparative view of the number of widowers and widows in different places.

Places.	Widowers.	Widows.
Manchefter,	432	1064
Salford,	89	149
Townfhip of ditto,	21	42
Parifh of Manchefter,	232	315
Monton,	14	13
Hale,	8	12
Horwich,	9	8
Darwen,	30	48
Cockey,	10	27
Chowbent,	26	43
Chinley, Brownfide, and Bugfworth,	15	18
Afhton under Line,	50	81
Parifh of ditto,	67	95
Total,	1003	1915.

Let no arguments in favour of polygamy be drawn from thefe tables. The practice is brutal; deftructive to friendfhip and moral fentiment; inconfiftent with one great end of marriage, the education of children; and fubverfive of the natural rights of more than half of the fpecies

—“ Higher of the genial bed by far.
“ And with myfterious reverence I deem.” MILTON.

Nor

Nor is this tyranny of man over the weaker, but more amiable ſex favourable to population. For notwithſtanding the number of females in the world may conſiderably exceed the number of males, yet there are more men capable of propagating their ſpecies, than women capable of bearing children. This painful office gradually becomes more dangerous and leſs frequent as the rigidneſs of the fibres increaſes, and ceaſes intirely at the age of fifty. The fatality of it is thus wiſely obviated, and the comforts of declining life are not interrupted by the arduous toil of nurſing. An inſtitution, therefore, which confines in ſervile bondage to one uſurper, many females in the prime of youth, muſt leave numbers deſtitute of the means, which nature hath pointed out, for perpetuating and increaſing the race of mankind. And it is a fact well known, that Armenia, in which a plurality of wives is not allowed, abounds more with inhabitants than any other province of the Turkiſh empire.

P. S. Since the preceding paper was written, the rev. Mr. CRADDOCK hath favoured me with a ſurvey of the town and pariſh of Aſhton under Line, diſtant about eight miles from Mancheſter. The inhabitants conſiſt of manufacturers and farmers.

An enumeration of the inhabitants of the town and parish of Ashton under Line, made in 1775.

	Town.	Parish.
Inhabitants,	2859	5097
Houses,	553	941
Families,	599	971
Males,	1406	2584
Females,	1453	2513
Married,	982	1679
Widowers,	50	67
Widows,	81	95
Under five years of age,	509	896
From five to ten,	396	764
ten to twenty,	541	1011
twenty to fifty,	1044	1882
fifty to seventy,	307	471
seventy to ninety,	62	73

The rev. Dr. PEPLOE, chancellor of the diocese of Chester, has honoured me with the following account of the parishes of Waverton and Tattenhall, both in the neighbourhood of Chester. The inhabitants are farmers and labourers.

An enumeration of the inhabitants of Tattenhall, made in Auguſt, 1774, by the rev. BRICE STORR, curate.

Inhabited houſes,	148
Uninhabited ditto,	2
Heads of families,	176
Aged above 14 years,	462
Men and boys,	382
Women and girls,	399

	Chriſtenings.	Burials.
1764	28	8
1765	21	9
1766	19	12
1767	29	11
1768	28	16
1769	24	15
1770	37	15
1771	30	9
1772	26	15
1773	38	20
	280	130

An enumeration of the inhabitants of Waverton, made in Auguſt, 1774, by the rev. Mr. BISSETT, miniſter of the pariſh.

Inhabited houſes,	109
Uninhabited ditto,	2
Heads of families,	116
Aged above 14 years,	406
Men and boys,	310
Women and girls,	322

	Chriſtenings.	Burials.
1764	19	10
1765	26	2
1766	17	7
1767	18	10
1768	22	10
1769	17	7
1770	20	8
1771	23	9
1772	18	12
1773	13	9
	193	84

In the valuable work which I have ſo often quoted, Dr. PRICE hath given many convincing and melancholy proofs of the declining ſtate of population in this kingdom. The growth of large towns; the prevalence of vice

vice and luxury; the difcouragements to marriage; the deftruction of cottages; and various other caufes have the moft unfavourable influence on the increafe of mankind. But it is to be hoped, that thefe evils do not univerfally prevail; and that even fome good may arife from them to check their baneful effects. Certain it is, that in this part of England the inhabitants multiply with great rapidity: and though the increafe may be chiefly owing to recruits drawn from other counties, yet the flourifhing ftate of our manufactures cannot fail to promote population, by affording plentiful means of fubfiftence to the poor. The bifhop of Chefter informs me, that in various parifh regifters which he has confulted, the births have progreffively become more numerous from generation to generation. At Boxley in Kent, where his lordfhip was vicar, he divided the times, from the commencement of the reign of queen ELIZABETH, into periods of twenty-one years; and found, that the number of births in the firft period was 310, and in the laft 525. The increafe was gradual through the whole time.

Manchefter,
Sept. 20, 1775.

IX. *Violent Asthmatic Fits, occasioned by the Effluvia of* Ipecacoanha. *By* William Scott, *M. D. of* Stamfordham, Northumberland.

R. Dec. 21, 1775.

MRS. S. of Stamfordham, in Northumberland, married a perſon of the medical faculty in the year 1759, being then about twenty-ſix years of age. She had been always remarkably healthy before that period, and quite free from all nervous or other complaints, except a trifling nervous head-ach that uſed to affect her temples and forehead, ſometimes for a night or ſo, about the time of her menſtruation. The firſt year or two after marriage ſhe enjoyed her uſual good health and ſpirits in general; but at times ſhe was afflicted with a very troubleſome ſhortneſs of breathing, attended with a remarkable ſtricture about her throat and breaſt, and with a particular kind of wheezing noiſe. Theſe fits came on very ſuddenly, and without any previous cauſe that at firſt could be aſſigned; and were often ſo violent as to threaten immediate ſuffocation. The duration of them was uncertain, ſometimes longer and ſometimes ſhorter; but in general they went off in two or three days, and commonly with a ſpitting of a tough phlegm, which, ſhe ſaid, had a diſagreeable metallic taſte. When theſe fits were off, ſhe enjoyed her uſual good health and ſpirits: had chil-

children; but suffered as little as any woman could do, either in breeding or lying-in; and it was not observed, that she was more subject to these fits when with child than at other times. She was blooded, and took some common pectoral medicines for them; but without any benefit. About a year and a half, or two years, after her marriage, she told her husband, that she observed these fits always attacked her when any *Ipecacoanha* was powdered in his shop; and that she was certain the effluvia of that medicine immediately brought them on. This was looked upon at first as a fancy, and little regard paid to it for some time. However, frequently after this, when any of that medicine was powdering or putting up, she used immediately to call out, perhaps from a different room, that she found the *ipecacoanha*, and that they would see her immediately affected by it. This I, and several others, saw frequently happen as she had said; so that we were at last convinced, to a demonstration, that the effluvia of the medicine, some how or other, so affected her nerves, as to bring on a very great and remarkable degree of spasm all about her throat and breast. Having thus had several repeated proofs of the effects the medicine had upon her, great precaution was therefore taken for several years never to pound any of it, but to purchase it powdered; and also care was taken, when weighing or putting any of it up, to send her out of the way, or to some distant part of the house. By these means she was kept pretty clear of it for seven or eight years together, during which time she enjoyed perfect good health.

Betwixt nine and ten o'clock in the evening, June 3, 1775, her huſband happening to have got a quantity of the *pulv. ipecacoanha* home, without conſidering opened it out, and put it into a bottle: his wife not being far off at the time, and then in perfect good health, immediately, almoſt even before it was got quite into the bottle, called out that ſhe felt the *ipecacoanha* affect her throat; on which ſhe was immediately ſeized with the ſtricture upon her breaſt and difficulty of breathing. She was adviſed to walk out into the air, to try if that would remove it; but it had little or no effect. She went to bed ſome little time afterwards; was exceedingly ill all night; and betwixt two and three o'clock next morning (June 4th) I ſaw her, when ſhe was gaſping for breath at a window, was as pale as death, her pulſe ſcarce to be felt, and in ſhort ſeemed evidently to be in the utmoſt danger of ſuffocation. She had ſeven or eight ounces of blood taken from her arm, her feet put into warm water, an anodyne draught with ſeven or eight drops of *laudanum* given her, and took frequently a table ſpoonful of oil of almonds. None of theſe ſeemed to have the leaſt effect: and ſhe continued much in the ſame way, with few or no intervals of eaſe, till about nine o'clock that morning; when, being in a manner almoſt exhauſted, ſhe fell into a kind of diſturbed ſleep, the difficulty of breathing with a wheezing noiſe ſtill continuing with little abatement. She ſlept ſome little time, and got out of bed again about eleven o'clock that forenoon; her breathing ſtill very difficult, and her eyes looked red and

a little inflamed. After ſhe got up, ſhe became eaſier towards the afternoon, and it was then ſuppoſed it would go off. Dr. BROWN, an eminent phyſician of Newcaſtle upon Tyne, happening to be in the neighbourhood, called upon Mrs. S.; and being told what had happened, ſaid he had known a caſe, pretty much ſimilar, from the ſame cauſe; and hoped, as ſhe then ſeemed better, it would ſoon go off; recommended to her riding out as ſoon ſhe was able, and to be kept open. Towards bed-time the ſame evening (June 4th) the difficulty of breathing returned, and ſhe was again exceedingly ill all night; had flannel cloths wrung out of warm water applied to her feet, breaſt, and throat, with little or no advantage; was blooded again about four o'clock next morning (June 5th), and had alſo a bliſter applied to the back part of her neck, ſtill continuing to take now and then a ſpoonful of the oil of almonds. She again fell into ſome ſleep about nine in the morning, and continued in bed till betwixt eleven and twelve: got up, and was again a little eaſier during the day; but at night was as bad as ever. And the ſame ſcene was continued for eight days and nights ſucceſſively; that is, ſhe was generally a little eaſier from about eleven o'clock of the forenoon, although ſtill far from well, till towards ten or eleven at night, when the ſhortneſs of breathing always returned very violently. However, after eight days ſhe began to get better reſt at nights; the aſthmatic fits were neither ſo long nor ſo violent; and in about fourteen days from the accident were almoſt entirely

gone off; and at the writing of this letter, Auguſt 1, 1775, although ſhe is now in very good health, ſhe has not yet quite recovered her uſual fleſh, ſtrength, and colour. Beſides the abovementioned medicines, ſhe took at times, during the firſt eight days, ſmall quantities of an emulſion of *ſperma ceti*, *lac. ammoniac.* and *ſucc. liquorit.*; had a doſe of cooling phyſic; rode and walked out a little ſometimes; had a few anodyne draughts with ſeven or eight drops of *laudanum*; but it could not be obſerved that ſhe got any benefit from them, except that ſhe ſometimes thought the oil of almonds gave her a little eaſe. She had a ſlight appearance of the *menſes* about four or five days after the accident happened, although it was then only about the middle of the uſual period; coughed up at times ſome ſmall quantities of blood, and had alſo ſome mixed with her ſtools and urine. The reaſon why the *laudanum*, the moſt effectual and univerſal anti-ſpaſmodic, was uſed in ſuch ſmall quantities, was, that it was known before that, ſhe could never bear above eight or nine drops of it, as the common doſe uſed to affect her with violent ſickneſs at ſtomach, giddineſs and pain in her head, &c. to ſo great a degree, that for ſome years paſt, ſhe neither would take, nor durſt her huſband adminiſter, a larger doſe to her. At the time the above accident happened, ſhe was neither with child, nor had had any for ſome years before.

The above effects of *ipecacoanba*, I believe, very ſeldom happen, and no doubt ariſe from ſome peculiarity of conſtitution. Medical writers, at leaſt as far as I can recollect

recollect at preſent, ſeem to have taken little or no notice of its ever producing ſuch an effect as the above. QUINCEY, however, if I remember right, mentions its producing aſthmas; but then he ſeems to mean, that it has that effect ſometimes when taken internally, but not by means of its effluvia.

Mr. LEIGHTON, a reputable ſurgeon and apothecary in Newcaſtle, told me, that the effluvia of *ipecacoanha* had the very ſame effect upon his wife, as it is above deſcribed to have had upon Mrs. S.; and that he had once, in particular, very near loſt her from having ſome of it powdered in his ſhop.

The *ipecacoanha* which had the above effects upon Mrs. S. was the common officinal aſh-coloured or grey kind.

Mrs. S. has now (Oct. 20, 1775) quite recovered her fleſh, ſtrength, colour, &c. I have ſometimes thought ſince, that perhaps muſk in pretty large doſes might have been of ſervice to her.

X. *An Account of the Succeſs of ſome Attempts to freeze Quickſilver, at* Albany Fort, *in* Hudſon's Bay, *in the Year* 1775: *with Obſervations on the Dipping-needle. By* Thomas Hutchins, *Eſquire, in a Letter to Dr.* Maty, *Sec. R. S.*

TO DR. MATY.

Albany Fort, Hudſon's Bay, Auguſt, 28, 1775.

SIR,

R. Dec. 21, 1775. I HAD made two or three more obſervations on the dipping-needle, but a violent fit of ſickneſs, which hath for ſome time attended me, and the having miſlaid many of my papers oblige me to ſend only theſe few to the Royal Society. I can aſſure you they were made with all poſſible circumſpection. I have ſent home the inſtruments by Captain CHRISTOPHER as deſired, and incloſe his receipt for them. The meteorological remarks have been made at all the places within my juriſdiction. I am very ſorry not to be able this year to ſend home thoſe from Albany, becauſe time will not permit me to copy them fair, and the ill ſtate of my health prevents it being done ſooner; but they ſhall certainly be ſent next year. The ſnow in my receiver during the whole winter was $64\frac{1}{2}$ inches.

Agreeable

According to the inſtructions you were ſo obliging to give me, concerning the congelation of quickſilver by cold, I made my firſt attempt to produce that extraordinary phænomenon on the 19th January, 1775. The thermometer at eight o'clock in the morning was at 37° below 0; but between ten and eleven it ſtood at 28°. I took the ſame thermometer and the beſt ſpare tube I had, which admitted only of 250° below 0, and immerſed them both together in a large tea cup filled with ſnow, and poured on *ſp. nitri fumans Glauberi* until the ſnow was diſſolved; but finding it did not cover the bulbs, I added more ſnow and ſpirit until the bulbs were entirely covered in the mixture, which was now liquified: the quickſilver ſubſided very gradually to 130°, and then ſtopped. I had another cup at hand, and mixed ſome ſnow and ſpirit in it ſo as to liquify the mixture, and removed both the thermometers into it; but found the ſtandard thermometer, by which I mean the inſtrument graduated by Meſſ. NAIRNE and BLUNT, London, had riſen in the removal to 110° below 0. As the mixture in this ſecond cup did not cover the bulbs, I added more as before, and alſo poured ſome out of the firſt cup. The ſpare tube, graduated by myſelf, ſtood in this cup at 130°; but the ſtandard fell deliberately to 263, where it ſtood again. I therefore prepared a third cup as before; the quickſilver did not aſcend in the removal, but when immerſed it fell very ſwiftly: that in the ſpare tube ſunk into the bulb, and that in the ſtandard deſcended much quicker than before, until it came to 400°; after,

after which it fell gently to 430°, and did not go beyond this point. As this was a greater degree of cold than that which Profeſſor BRAUM ſaid quickſilver would freeze at, I determined to break my ſpare tube, which was eaſily done by a ſtroke with a pair of ſciſſars; the quickſilver in a fall of about ſix inches was flattened, and ſome *globuli* appeared at the bottom of a tea cup in which it was received. This occaſioned Mr. JARVIS the ſurgeon, who was ſo obliging as to aſſiſt me, to exclaim the quickſilver was not frozen; but when he ſaw me repeatedly ſtrike the cake with an hammer, and heard it give a deadiſh ſound like lead, as M. BRAUM juſtly expreſſes it, he receded from his firſt opinion. The quickſilver liquified in about ſix or ten ſeconds. The ſurface, when frozen, was finely poliſhed. I imagine the internal part of the globe was not frozen, and that the force of the fall having flattened it, might crack the external coat or ſhell of congealed quickſilver, and permit the *globuli* which I ſaw to eſcape into the cup. On taking the ſtandard thermometer out of the mixture it fell 10° lower than when the bulb was immerſed; but it ſoon began to aſcend, and being taken into my room, it roſe to 40 above 0, when I replaced it a little before noon. The operation laſted about thirty or forty minutes.

Having ſucceeded thus far in my firſt attempt to freeze quickſilver, I was anxious for another opportunity; but ſometimes buſineſs, and want of a ſufficient degree of cold in the air at other times, obliged me to defer it until the 11th of February, which was very clear, and the

ther-

thermometer ſtood at 36° below o. I began the operation exactly at forty-five minutes paſt eight: the inſtrument being at 28° was put into a large tea-cup with the mixture as above, together with a ſpare tube, graduated by myſelf; the quickſilver in the latter ſubſided into the bulb, which was only 200° below o; in the ſtandard thermometer it ſunk to 447° at fifty-nine minutes after eight o'clock, which gave me great hopes of ſucceeding ſtill better than in my firſt attempt, becauſe I had now a greater degree of cold in my firſt cup than I had before in my third. Finding it did not go any lower, I removed it into a ſecond cup, prepared as before; but the quickſilver ſhewed no alteration in it. After waiting a conſiderable time, I removed it into a third; but in the removal, the quickſilver roſe to 380° below o. I imagined I had put in too much ſpirit in proportion to the ſnow, and therefore added more of the latter, by which means it ſubſided to 408°; and after ſtanding at this point for ſome time, it roſe to 406°; and ſoon after, at ten minutes after nine o'clock, it roſe with great celerity and full of bubbles, until it came to 160° above o, and in a minute after it reached the point of boiling water. On examining the inſtrument, I found the bulb cracked and the quickſilver fluid, to my ſurprize and regret.

REMARKS.

I imagine it is extremely difficult to aſcertain the exact degree at which quickſilver begins to freeze, becauſe no particular alteration or circumſtance points out the

moment of congelation, or even afterwards; for the quickſilver in the tube ſtill continues to fall, and hath the ſame appearance as before, contrary to what we obſerve in water. I think, therefore, it can only be determined by breaking the glaſſes at different altitudes; but this would be both tedious and expenſive. However, were ſpare tubes filled by the maker, and graduated by the operator, to be made uſe of, the expence would be leſs; but then, if thoſe tubes will not admit of being graduated to a conſiderable diſtance (ſuppoſe 1000°) below 0, the operator is obliged to put a thermometer, with a ſcale graduated by the inſtrument-maker, together with the other tube, into the mixture, to learn the degree of cold after the quickſilver in the ſpare tube, deſigned chiefly for the experiment, hath ſubſided into the bulb, as was my caſe. Profeſſor BRAUM made it ſubſide even to 1500°, which ſhews the fineneſs of the tubes he made uſe of.

Theſe, SIR, are all the obſervations that much buſineſs, and an infirm ſtate of health, permit me at this time to tranſmit to you. I wiſh they were more worthy your attention. You are the beſt judge whether they will bear the inſpection of the Royal Society, and to your candour I ſubmit them. I propoſe making ſome more experiments this year, which I ſhall take a pleaſure in communicating; being, with great regard, &c.

Obſer-

Obſervations on the Dipping-needle, at Albany Fort, Longitude 83° 30′ Weſt. Latitude 52° 24′ North.

February 3, 1775.

°	′	
76	50	Index Eaſt.
77	12	
80	0	Index Weſt.
78	45	
79	20	changed the poles, the index ſtill Weſt.
79	47	
79	40	Index Eaſt.
79	34	
79	45	
79	17	
79	8	Index Weſt.
78	50	
79	45	changed the poles again, the index Weſt.
79	19	
81	8	Index Eaſt.
79	45	
81	25	Mean of all obſervations 79° 17′$\frac{5}{8}$.

I took particular care in placing the inſtrument in the magnetic meridian, and was near four hours before I got it right. The obſervations employed me four hours more; and when they were finiſhed, I turned the index South, when the needle pointed at 89° 56′, or very near perpendicular. I cannot poſſibly account for the differences, more eſpecially as I took ſo much pains to render theſe obſervations correct. Ob-

March 13, 1775.

°	′	
77	45	Index East.
80	45	
80	5	
78	40	
79	55	Index West.
80	6	
79	45	
80	8	
78	10	Poles changed, and index West.
79	50	
78	30	
78	50	
80	45	Index East.
78	45	
80	15	
78	20	

Mean of the whole is 79° $25\frac{1}{4}$′.

May 6, 1775.

79	0	Index Weſt.
78	0	
79	45	
79	15	Index Eaſt.
79	15	
79	45	
78	30	Poles changed, index ſtill Eaſt,
80	35	
80	10	
79	45	Index Weſt.
80	5	
79	40	

Mean 79° 28′,75.

Theſe obſervations were made in the open air, on a platform on the top of the fort.

XI. *Aſtro-*

XI. *Aſtronomical Obſervations made in the* Auſtrian Netherlands *in* 1772 *and* 1773. *By* Nathanael Pigott, *Eſquire, F. R. S. Foreign Member of the Academies of* Bruſſels *and* Caen. *In a Letter to the Reverend* Nevil Maſkelyne, *Aſtronomer Royal, F. R. S.*

TO THE REV. NEVIL MASKELYNE.

Louvain,
Auguſt 11, 1775.

REV. SIR,

R. Dec. 9, 1775. I RECEIVED, about a month ago, the favour of your letter, and return you many thanks for the Greenwich obſervations, which you were ſo obliging to ſend me. I wait, with impatience, the publication of your journey into Scotland, which muſt be very curious and intereſting. I beg of you, SIR, to preſent my reſpects to the Royal Society, with the incloſed aſtronomical obſervations, which I have contracted as much as I well could, conſiſtently with a view of affording means to verify them, or rectify any miſtake which, by inadvertency, may have crept in. I ſhall only add a ſhort account of the inſtruments I uſed, and the elements I employed in the calculations, that a proper judgement may be formed, how far theſe obſervations may be depended upon.

The meridian altitudes were taken with a quadrant one foot radius made by Mr. BIRD, very ſteadily fixed; free from any communication with the floor, and well placed in the plane of the meridian.

Repeated obſervations for the error of the line of collimation gave 1′ 58″,7 additive to the zenith diſtances.

I always obſerved both the limbs of the Sun on the meridian, when the weather would permit: its declination was computed from the Nautical Almanac: its parallax and all refractions, account being always kept of the height of the barometer and thermometer, from Profeſſor MAYER's tables, publiſhed by the Board of Longitude.

The declinations of the fixed ſtars were taken from the *Connoiſſance des tems.* The corrections on account of aberration and nutation, were either taken from the ſame ephemeris, or computed.

My ſon always obſerved ♃'s ſatellites with a reflector, made by SHORT, of eighteen inches focal length, magnifying 95 times. I obſerved them with a reflector of two feet and a half focal length, made by WING, magnifying the diameter of the object about 200 times.

The clock was a compound gridiron pendulum, made by the Sieur LE PAUTE at Paris.

The equal altitudes were taken with a quadrant of eighteen inches radius.

This aſtronomical journey was undertaken at the requeſt of the government here. They expreſſed a deſire that the ſituations of ſome of their towns, at leaſt, ſhould be determined by obſervation; and I readily concurred, without regretting either trouble or expence, in a project which had public utility in view.

I am, &c.

Cor-

Corresponding altitude of the Sun and Stars.

			h	′	″
1772					
Aug.	30.	Clock at noon corrected by four obs. of Sun,	11	54	7,9
Sept.	5.	Clock at noon corrected by seven ditto,	11	47	26,8
	13.	Clock at noon corrected by four ditto,	0	2	48,5
	19.	Clock at noon corrected by seven ditto,	11	57	21,1
	21.	Clock at noon corrected by seven ditto,	11	54	42,7
Oct.	10.	Clock at noon corrected by eight ditto,	11	32	37,7
	11.	Clock at noon corrected by nine ditto,	11	31	20,0
	19.	Clock at noon corrected by five ditto,	11	22	33,1
	20.	Clock at noon corrected by five ditto,	11	21	31,c
	21.	Clock at noon corrected by six ditto,	11	20	30,5+
Nov.	9.	Clock at noon corrected by five ditto,	0	21	23,6—
	11.	Clock at noon corrected by three ditto,	0	23	10+
	13.	Clock at noon corrected by three sets ditto,	0	24	55,1
	14.	Clock at noon corrected by six ditto,	0	25	9,7+
	20.	Clock at noon corrected by seven ditto,	0	30	30,5—
	Ditto.	Fomahant crossed the meridian by the clock,	7	28	28,0
	21.	Fomahant crossed the meridian by ditto,	7	25	19,9—
Dec.	23.	β Ceti on meridian by the clock,	6	39	38,5—
	24.	Clock at noon corrected by four obs. of Sun,	0	19	6,0

NAMUR

NAMUR 1772.

By a mean of eight meridian altitudes of the fixed ſtars taken in September, I determined the latitude of my obſervatory, in the *Rue St. Nicholas*, near the Recollets Church, 50° 28′ 32″ North.

Sept. 4, emerſion of ♃'s firſt ſat. at 10^h 38′ by the clock.

	Apparent times. h ′ ″
Emerſion at Tyrnaw in Hungary, by Father WEISS, 3½ feet achrom. weather fine,	11 41 11
To reduce to time of the Royal Obſervatory at Paris,	—1 0 55
Emerſion of the ſat. at Paris,	10 40 16
Emerſion by my ſon at Namur,	11 49 55
Namur Eaſt of the Paris Obſervatory, in time,	9 39

Or in parts of a great circle 2° 24′ 45″.

A very good obſervation. ♃'s belts very diſtinct. I ſaw the emerſion 5″ latter.

LUXEMBOURG 1772.

By a mean of twenty-nine meridian altitudes of the Sun and fixed ſtars taken in September and October, one of which only, gives the latitude 19,″9 different from the mean of the whole, I determined the latitude of my obſervatory, in the *Rue St. Eſprit*, near the Jeſuits Church, 49° 37′ 6″+ North,

Sept. 11. emerſion ♃'s firſt ſat. 12^h 55′ 20″ by the clock.

	Apparent times. h ′ ″
Emerſion at Greenwich, by the Nautical Almanac,	12 28 15
Error of tables, by a good obſervation at Tyrnaw, Sept. 4,	+0 7
Greenwich Weſt of Paris,	+9 16
Emerſion at Paris,	12 37 38
I obſerved it at Luxembourg,	12 53 19
Luxembourg Eaſt of Paris Obſervatory,	15 41

My ſon ſaw the emerſion 19″ later.

LUXEMBOURG, Sept. 20, 1772.

Emerſion ♃'s firſt ſat. at 9h 15′ 23″ by the clock.

	Apparent times. h ′ ″
Emerſion at Greenwich, by Nautical Almanac,	8 55 12
Error of tables, as above,	+ 7
Greenwich Weſt of Paris,	+ 9 16
Emerſion at Paris,	9 4 35
I obſerved it at Luxembourg,	9 19 52
Luxembourg Eaſt of Paris,	15 17

My ſon obſerved the emerſion 13″ later.

The ſame emerſion.

Emerſion at Greenwich, by Nautical Almanac,	8 55 12
Error of tables, by an obſervation at Greenwich on the 27th,	— 25
At Greenwich, by the tables corrected,	8 54 47
Difference of meridians,	+ 9 16
Emerſion at Paris,	9 4 3
I obſerved it at Luxembourgh,	9 19 52
Luxembourg Eaſt of Paris,	15 49

LUXEMBOURG, Oct. 19, 1772.

Emerſion ♃'s ſecond ſat. at 7h 31′ 39″ by the clock.

	Apparent time. h ′ ″
Emerſion at Tyrnaw, achrom. 3½ feet,	8 54 47
To reduce to Paris time,	—1 0 55
At Paris,	7 53 52
My ſon obſerved it at Luxembourg,	8 9 14
Luxembourg Eaſt of Paris,	15 22

I ſaw it 3″ later.

The ſame emerſion.

At Senones, by M. MESSIER, achrom. 5 feet,	8 12 8
To reduce to Paris time, according to his letter,	— 18 34
Emerſion at Paris,	7 53 34
At Luxembourg,	8 9 14
Luxembourg Eaſt of Paris,	15 40

This emerſion was obſerved alſo at 7h 44′ 13″ apparent time, with a 6 feet reflector, at Greenwich. Allowing, according to the Aſtronomer Royal's rule, about 20″ for the difference of teleſcopes, the reſult will be, Luxembourg Eaſt of Paris 15′ 25″.

LUXEMBOURG, Oct. 20, 1772.

Emerſion ♃'s firſt ſat. at 11h 0′ 15″ by the clock.

	Apparent time.
	h ′ ″
Emerſion at Greenwich, achrom. 3½ feet,	11 14 18
To reduce to Paris time,	+ 9 16
At Paris,	11 23 34
I obſerved it at Luxembourg,	11 39 13
Luxembourg Eaſt of Paris,	15 39

My ſon ſaw it 12″ later.

LUXEMBOURG, Oct. 11, 1772.

Eclipse of the Moon.

By clock h ′ ″	Appar. time. h ′ ″		Luxemb. East of Paris. ′ ″
6 25 18	6 54 17	Galileus out of the shadow at Luxembourg,	
6 36 57	7 5 4	Copernicus begins to emerge at Luxembourg,	
6 38 0	7 6 59	Copernicus out at Luxembourg,	
	6 51 37	at Senones, reduced to Paris,	15 22
	6 51 54	at the Observatory at Paris,	15 5
6 40 42	7 9 41	Tycho begins to emerge at Luxembourg,	
6 42 35	7 11 34	Tycho out at Luxembourg,	
	6 56 13	at Senones, reduced to Paris,	15 21
	6 55 56	at the Observatory at Paris,	15 38
	6 56 21	Rue St. Honoré at Paris, by M. DE LA LANDE,	15 13
6 53 56	7 22 54	Manilius begins to emerge at Luxembourg,	
	7 7 25	at the Observatory at Paris,	15 29
	7 7 28	Paris, Rue St. Honoré,	15 26
7 3 25	7 32 25	Mare Serenitatis out at Luxembourg,	
	7 17 10	Senones, reduced to Paris,	15 15
	7 17 23	at the Observatory at Paris,	15 2
7 12 17	7 41 17	Mare Crisium begins to emerge at Luxembourg,	
	7 26 18	at Paris, Rue St. Honoré,	14 59
7 17 37	7 46 35	Mare Crisium out at Luxembourg,	
	7 30 45	at Senones, reduced to Paris,	15 50
		By a mean,	15 20+
	7 52 7	end of the eclipse at Luxembourg.	
	7 33 55	at Senones, by M. MESSIER, reduced to Paris.	
	7 35 46	at the Royal Observatory at Paris.	
	7 34 54	Paris, Rue St. Honoré, by M. DE LA LANDE.	

At Luxembourg, Sky remarkably clear, without the least wind.

Hence, by a mean of ♃'s satellites,	15 33+
Luxembourg East of the R. Observ. at Paris, by a mean of the whole,	15 27+

Which gives $3^\circ\ 51'\ 45''$ in parts of a great circle.

LUXEM-

LUXEMBOURG, 1772.

Oct. 22. at 3 hours P. M. a magnetic needle of four inches, made by DOLLOND, gave the declination Weſt 18° 42′½.

Oct. 23. at 10 hours A. M. the declination was 18° 50′.

At LA HEESE, near HOOGSTRAETEN.

By a mean of twenty-two meridian altitudes of the Sun and fixed ſtars taken in November 1772, one of which only, gives the latitude different from the mean of the whole 10″,2, I determined the latitude of my obſervatory 51° 23′ 2″+N.

Nov. 9. Emerſion ♃'s third ſat. 6h 49′ 29″ by the clock.

	Apparent time. h ′ ″
Emerſion at Tyrnaw, achrom. 3½ feet,	7 19 2
To reduce to Paris time,	—1 0 55
Emerſion at Paris,	6 18 7
I obſerved it at La Heeſe,	6 27 50
La Heeſe Eaſt of the Paris Obſervatory,	9 43

My ſon ſaw the Emerſion 13″ later.

LA HEESE, Nov. 14, 1772.

Emerſion ♃'s firſt ſat. at 6h 44′ 5″ by the clock.

	Apparent time. h ′ ″
Emerſion at Senones, achrom. 3½ feet,	6 27 7
To reduce to Paris time,	— 18 34
Emerſion at Paris,	6 8 33
I obſerved it at La Heeſe,	6 18 42
La Heeſe Eaſt of Paris,	10 9

The ſame emerſion.

At Greenwich, 6 feet reflector,	5 59 28
Difference of teleſcopes,	+ 0 15
Greenwich Weſt of Paris,	+ 9 16
Emerſion at Paris,	6 8 59
By my obſervation at La Heeſe,	6 18 42
La Heeſe Eaſt of Paris,	9 43

Nov. 20. Emerſion ♃'s ſecond ſat. at 8h 15′ 59″ by the clock.

Emerſion by my ſon, but I have no correſponding obſevation,	7 45 9

At

At LA HEESE, Nov. 21, 1772.

Emerſion ♃'s firſt ſat. at 8h 45′ 12″ by the clock.

	Apparent time. h ′ ″
Emerſion at Greenwich, by Nautical Almanac,	7 53 54
Error of tables, by the obſerv. at Greenwich, Senones, and Tyrnaw,	+ 12
Greenwich Weſt of Paris,	+ 9 16
Emerſion at Paris,	8 3 22
I obſerved it at La Heeſe,	8 13 3
La Heeſe Eaſt of Paris,	9 41

My ſon ſaw the emerſion 5″ later.

By a mean of the obſervations of ♃'s ſatellites, *La Heeſe* is Eaſt of the Royal Obſervatory at Paris 9′ 49″ in time, or 2° 27′ 15″. If the obſervation of Nov. 14, compared with that made at *Senones*, be rejected, the difference of meridians will be $7\frac{1}{2}''$ in time leſs.

At HOOGSTRAETEN, Nov. 24, 1772.

By a base of 3028 feet, twice very exactly measured and angles taken with a quadrant 18 inches radius, I determined the church of *Hoogstraeten* 10380 feet North and 5873 feet East of the Observatory at *La Heese*.

	°	′	″
Hence latitude of La Heese,	51	23	2+
Difference of latitudes,		+ 1	42
Latitude North of the church of Hoogstraeten,	51	24	44

	h	′	″
Longitude of La Heese, as above,	0	9	49
Difference of meridians,			+ 6
Hoogstraeten East of the Royal Observatory at Paris, in time,		9	55

Or 2° 28′ 45″ in parts of a great circle.

At OSTENDE.

By a mean of 24 meridian altitudes of the Sun and stars taken in December, one of which only, gives the latitude 11″,7 different from the mean of the whole, I determined the latitude of my observatory, in the *Rue de la Poste* 51° 15′ 10″ North.

The *Connoissance des tems* gives the lat. 51° 13′ 55″; but I do not know in what part of the town, or by whom it was determined.

At

At OSTENDE, Dec. 23, 1772.

Emerſion ♃'s firſt ſat. at 4h 52′ 48″ by the clock.

	Apparent time. h ′ ″
Emerſion at Greenwich, by Nautical Almanac,	4 22 1
Error of tables, as Nov. 21,	+ 12
Greenwich Weſt of Paris,	+ 9 16
Emerſion at Paris,	4 31 29
I obſerved it at Oſtende,	4 34 2
Oſtende Eaſt of Paris Obſervatory, in time,	2 33

Or 38′ 15″ in parts of a great circle.

Twilight ſtrong; ſky very clear and ſerene; good obſervation.

Dec. 24, at 3 hours P. M. I found the declination of the magnetic needle Weſt 20° 35½′.

At TOURNAI, 1773.

By a mean of 14 meridian altitudes of the Sun and ſtars taken in January, one of which only gives the latitude 22″,8 different from the mean of the whole, I determined the latitude of my obſervatory in the *Rue des Jeſuites* 50° 36′ 57″+ North.

The weather would not permit to obſerve either ♃'s ſatellites, or an occultation of a ſtar by the Moon, for the longitude of Tournai.

XII. *An Account of ſome Attempts to imitate the Effects of the* Torpedo *by Electricity. By the Hon.* Henry Cavendiſh, *F. R. S.*

R. Jan. 18, 1775.

ALTHOUGH the proofs brought by Mr. WALSH, that the phenomena of the torpedo are produced by electricity, are ſuch as leave little room for doubt; yet it muſt be confeſſed, that there are ſome circumſtances, which at firſt ſight ſeem ſcarcely to be reconciled with this ſuppoſition. I propoſe, therefore, to examine whether theſe circumſtances are really incompatible with ſuch an opinion; and to give an account of ſome attempts to imitate the effects of this animal by electricity.

It appears from Mr. WALSH's experiments, that the torpedo is not conſtantly electrical, but hath a power of throwing at pleaſure a great quantity of electric fluid from one ſurface of thoſe parts which he calls the electrical organs to the other; that is, from the upper ſurface to the lower, or from the lower to the upper, the experiments do not determine which; by which means a ſhock is produced in the body of a perſon who makes any part of the circuit which the fluid takes in its motion to reſtore the equilibrium.

One

One of the principal difficulties attending the ſuppoſition, that theſe phenomena are produced by electricity, is, that a ſhock may be perceived when the fiſh is held under water; and in other circumſtances, where the electric fluid hath a much readier paſſage than through the perſon's body. To explain this, it muſt be conſidered, that when a jar is electrified, and any number of different circuits are made between its poſitive and negative ſide, ſome electricity will neceſſarily paſs along each; but a greater quantity will paſs through thoſe in which it meets with leſs reſiſtance, than thoſe in which it meets with more. For inſtance, let a perſon take ſome yards of very fine wire, holding one end in each hand, and let him diſcharge the jar by touching the outſide with one end of the wire, and the inſide with the other; he will feel a ſhock, provided the jar is charged high enough; but leſs than if he had diſcharged it without holding the wire in his hands; which ſhews, that part of the electricity paſſes through his body, and part through the wire. Some electricians indeed ſeem to have ſuppoſed that the electric fluid paſſes only along the ſhorteſt and readieſt circuit; but beſides that ſuch a ſuppoſition would be quite contrary to what is obſerved in all other fluids, it does not agree with experience. What ſeems to have led to this miſtake is, that in diſcharging a jar by a wire held in both hands, as in the above mentioned experiment, the perſon will feel no ſhock, unleſs either the wire is very long and ſlender, or the jar is very large and highly charged. The reaſon of which is, that metals conduct fur-

ſurprizingly better than the human body, or any other ſubſtance I am acquainted with; and conſequently, unleſs the wire is very long and ſlender, the quantity of electricity which will paſs through the perſon's body will bear ſo ſmall a proportion to the whole, as not to give any ſenſible ſhock, unleſs the jar is very large and highly charged.

It appears from ſome experiments, of which I propoſe ſhortly to lay an account before this Society, that iron wire conducts about 400 million times better than rain or diſtilled water; that is, the electricity meets with no more reſiſtance in paſſing through a piece of iron wire 400,000,000 inches long, than through a column of water of the ſame diameter only one inch long. Sea-water, or a ſolution of one part of ſalt in 30 of water, conducts 100 times, and a ſaturated ſolution of ſea ſalt about 720 times better than rain water.

To apply what hath been here ſaid to the torpedo; ſuppoſe the fiſh by any means to convey in an inſtant a quantity of electricity through its electric organs, from the lower ſurface to the upper, ſo as to make the upper ſurface contain more than its natural quantity, and the lower leſs; this fluid will immediately flow back in all directions, part over the moiſt ſurface, and part through the ſubſtance of its body, ſuppoſing it to conduct electricity, as in all probability it does, till the equilibrium is reſtored; and if any perſon hath at the time one hand on the lower ſurface of the electric organs, and the other on the upper, part of the fluid will paſs through his body.

More-

Moreover, if he hath one hand on one ſurface of an electric organ, and another on any other part of its body, for inſtance the tail, ſtill ſome part of the fluid will paſs through him, though much leſs than in the former caſe; for as part of the fluid, in its way from the upper ſurface of the organ to the lower, will go through the tail, ſome of that part will paſs through the perſon's body. Some fluid alſo will paſs through him, even though he does not touch either electric organ, but hath his hands on any two parts of the fiſhes body whatever, provided one of thoſe parts is nearer to the upper ſurface of the electric organs than the other. On the ſame principle, if the torpedo is immerſed in water, the fluid will paſs through the water in all directions, and that even to great diſtances from its body, as is repreſented in fig. 1. where the full lines repreſent the ſection of its body, and the dotted lines the direction of the electric fluid; but it muſt be obſerved, that the nearer any part of the water is to the fiſhes body, the greater quantity of fluid will paſs through it. Moreover, if any perſon touches the fiſh in this ſituation, either with one hand on the upper ſurface of an electric organ, and the other on the lower, or in any other of thoſe manners in which I ſuppoſed it to be touched when out of the water, ſome fluid will paſs through his body; but evidently leſs than when the animal is held in the air, as a great proportion of the fluid will paſs through the water: and even ſome fluid will paſs through him, though he does not touch the fiſh at all; but only holds

his hands in the water, provided one hand is nearer to the upper ſurface of the electric organs than the other.

The ſecond difficulty is, that no one hath ever perceived the ſhock to be accompanied with any ſpark or light, or with the leaſt degree of attraction or repulſion. With regard to this, it muſt be obſerved, that when a perſon receives a ſhock from the torpedo, he muſt have formed the circuit between its upper and lower ſurface before it begins to throw the electricity from one ſide to the other; for otherwiſe the fluid would be diſcharged over the ſurface of the fiſhes body before the circuit was compleated, and conſequently the perſon would receive no ſhock. The only way, therefore, by which any light or ſpark could be perceived, muſt be by making ſome interruption in the circuit. Now Mr. WALSH found, that the ſhock would never paſs through the leaſt ſenſible ſpace of air, or even through a ſmall braſs chain. This circumſtance, therefore, does not ſeem inconſiſtent with the ſuppoſition that the phenomena of the torpedo are owing to electricity; for a large battery will give a conſiderable ſhock, though ſo weakly charged that the electricity will hardly paſs through any ſenſible ſpace of air; and the larger the battery is, the leſs will this ſpace be. The principle on which this depends will appear from the following experiments.

I took ſeveral jars of different ſizes, and connected them to the ſame prime conductor, and electrified them in a given degree, as ſhewn by a very exact electrometer; and then found how near the knobs of an inſtrument

in

in the nature of Mr. LANE's electrometer muſt be approached, before the jars would diſcharge themſelves. I then electrified the ſame jars again in the ſame degree as before, and ſeparated all of them from the conductor except one. It was found, that the diſtance to which the knobs muſt be approached to diſcharge this ſingle jar was not ſenſibly leſs than the former. It was alſo found, that the divergence of the electrometer was the ſame after the removal of the jars as before, provided it was placed at a conſiderable diſtance from them: from which laſt circumſtance, I think we may conclude, that the force with which the fluid endeavours to eſcape from the ſingle jar is the ſame as from all the jars together.

It appears, therefore, that the diſtance to which the ſpark will fly is not ſenſibly affected by the number or ſize of the jars, but depends only on the force with which they are electrified; that is, on the force with which the fluid endeavours to eſcape from them: conſequently, a large jar, or a great number of jars, will give a greater ſhock than a ſmall one, or a ſmall number, electrified to ſuch a degree, that the ſpark ſhall fly to the ſame diſtance; for it is well known, that a large jar, or a great number of jars, will give a greater ſhock than a ſmall one, or a ſmall number, electrified with the ſame force.

In trying this experiment, the jars were charged very weakly, inſomuch that the diſtance to which the ſpark would fly was not more than the 20th of an inch. The electrometer I uſed conſiſted of two ſtraws, 10 inches long, hanging parallel to each other, and turning at one end

end on ſteel pins as centers, with cork balls about $\frac{1}{4}$ of an inch in diameter fixed on the other end. The way by which I eſtimated the divergence of theſe balls, was by ſeeing whether they appeared to coincide with parallel lines placed behind them at about ten inches diſtance; taking care to hold my eye always at the ſame diſtance from the balls, and not leſs than thirty inches off. To make the ſtraws conduct the better, they were gilded, which cauſes them to be much more regular in their effect. This electrometer is very accurate; but can be uſed only when the electricity is very weak. It would be eaſy, however, to make one on the ſame principle, which ſhould be fit for meaſuring pretty ſtrong electricity.

The inſtrument by which I found to what diſtance the ſpark would fly is repreſented in fig. 2.; it differs from Mr. LANE's electrometer no otherwiſe than in not being fixed to a jar, but made ſo as to be held in the hand. The part ABCDEFGKLM is of baked wood, the reſt of braſs; the part GKL being covered with tinfoil communicating with the braſs work at FG; and the part ABM being alſo covered with a piece of tinfoil, communicating with the braſs work at CD.

I next took four jars, all of the ſame ſize; electrified one of them to a given degree, as ſhewn by the electrometer; and tried the ſtrength of the ſhock which it gave; and found alſo to what diſtance the ſpark would fly. I then took two of the jars, electrified them in the ſame degree as before, and communicated their electricity to the two remaining. The ſhock of theſe four jars united, was

was rather greater than that of the ſingle jar; but the diſtance to which the ſpark would fly was only half as great.

Hence it appears, that the ſpark from four jars, all of the ſame ſize, will not dart to quite half ſo great a diſtance as that from one of thoſe jars electrified in ſuch a degree as to give a ſhock of equal violence; and conſequently the diſtance to which the ſpark will fly is inverſely in a rather greater proportion than the ſquare root of the number of jars, ſuppoſing them to be electrified in ſuch a degree that the ſhock ſhall be of a given ſtrength. It muſt be obſerved, that in the laſt mentioned experiment, the quantity of electric fluid which paſſed through my body was twice as great in taking the ſhock of the four jars, as in taking that of the ſingle one; but the force with which it was impelled was evidently leſs, and I think we may conclude, was only half as great. If ſo, it appears that a given quantity of electricity, impelled through our body with a given force, produces a rather leſs ſhock than twice that quantity, impelled with half that force; and conſequently, the ſtrength of the ſhock depends rather more on the quantity of fluid which paſſes through our body, than on the force with which it is impelled.

That no one could ever perceive the ſhock to be accompanied with any attraction or repulſion, does not ſeem extraordinary; for as the electricity of the torpedo is diſſipated by eſcaping through or over the ſurface of its body, the inſtant it is produced, a pair of pith balls

ſuſpended from any thing in contact with the animal will not have time to ſeparate, nor will a fine thread hung near its body have time to move towards it, before the electricity is diſſipated. Accordingly I have been informed by Dr. PRIESTLEY, that in diſcharging a battery he never could find a pair of pith balls ſuſpended from the diſcharging rod to ſeparate. But, beſides, there are ſcarce any pith balls ſo fine, as to ſeparate when ſuſpended from a battery ſo weakly electrified that its ſhock will not paſs through a chain, as is the caſe with that of the torpedo.

In order to examine more accurately, how far the phenomena of the torpedo would agree with electricity, I endeavoured to imitate them by means of the following apparatus. ABCFGDE fig. 3. is a piece of wood, the part ABCDE of which is cut into the ſhape of the torpedo, and is $16\frac{3}{4}$ inches long from A to D, and $10\frac{3}{4}$ broad from B to E; the part CFGD is 40 inches long, and ſerves by way of handle. MN*nm* is a glaſs tube let into a groove cut in the wood. W*w* is a piece of wire paſſing through the glaſs tube, and ſoldered at W to a thin piece of pewter R*r* lying flat on the wood, and intended to repreſent the upper ſurface of the electric organs. On the other ſide of the wood there is placed ſuch another glaſs tube, not repreſented in the figure, with a wire paſſing through it, and ſoldered to another piece of pewter of the ſame ſize and ſhape as R*r* intended to repreſent the lower ſurface of thoſe organs. The whole part ABCDE is covered with a piece of ſheep's ſkin leather.

 In

In making experiments with this inſtrument, or artificial torpedo as I ſhall call it, after having kept it in water of about the ſame ſaltneſs as that of the ſea, till thoroughly ſoaked, I faſtened the end of one of the wires, that not repreſented in the drawing for example, to the negative ſide of a large battery, and when it was ſufficiently charged, touched the poſitive ſide with the end of the wire W*w*; by which means the battery was diſcharged through the torpedo: for as the wires were incloſed in glaſs tubes, which extended about an inch beyond the end of the wood FG no electricity could paſs from the poſitive ſide of the battery to the negative, except by flowing along the wire W*w* to the pewter R*r*, and thence either through the ſubſtance of the wood, or along the wet leather, to the oppoſite piece of pewter, and thence along the other wire to the negative ſide. When I would receive a ſhock myſelf, I employed an aſſiſtant to charge the battery, and when my hands were in the proper poſition, to diſcharge it in the abovementioned manner by means of the wire W*w*. In experiments with this torpedo under water, I made uſe of a wooden trough; and as the ſtrength of the ſhock may, perhaps, depend in ſome meaſure on the ſize of the trough, and on the manner in which the torpedo lies in it, I have in fig. 4. given a vertical ſection of it; the torpedo being placed in the ſame ſituation as in the figure. ABCDE is the trough; the length BC is 19 inches; the depth AB is 14; and the breadth is 13; conſequently, as the torpedo is two inches thick in the thickeſt part, there is about

$5\frac{1}{2}$

$5\frac{1}{2}$ inches diſtance between its ſides and thoſe of the trough.

The battery was compoſed of 49 jars, of extremely thin glaſs, diſpoſed in 7 rows, and ſo contrived that I could uſe any number of rows I choſe. The outſides of the jars were coated with tinfoil; but as it would have been very difficult to have coated the inſides in that manner, they were filled with ſalt water. In a battery to anſwer the purpoſe for which this was intended, it is evidently neceſſary that the metals ſerving to make the communications between the different jars ſhould be joined quite cloſe: accordingly care was taken that the contacts ſhould be made as perfect as poſſible. I find, by trial, that each row of the battery contains about $15\frac{3}{4}$ times as much electricity, when both are connected to the ſame prime conductor, as a plate of crown glaſs, the area of whoſe coating is 100 ſquare inches, and whoſe thickneſs is $\frac{55}{1000}$ of an inch; that is, ſuch that one ſquare foot of it ſhall weigh 10 oz. 12 pwts.; and conſequently, the whole battery contains about 110 times as much electricity as this plate (a).

The way by which this was determined, and which, I think, is one of the eaſieſt methods of comparing the quantity of electricity which different batteries will receive

(a) I find, by experiment, that the quantity of electricity which coated glaſs of different ſhapes and ſizes will receive with the ſame degree of electrification, is directly as the area of the coating, and inverſely as the thickneſs of the glaſs; whence the proportion which the quantity of electricity in this battery bears to that in a glaſs or jar of any other ſize, may eaſily be computed.

ceive with the ſame degree of electrification, was this: Firſt of all, ſuppoſing a jar or battery to be electrified till the balls of the abovementioned electrometer ſeparated to a given diſtance, I found how much they would ſeparate when the quantity of electricity in that jar or battery was reduced to one-half. To do this, I took two jars, as nearly equal as poſſible, and electrified one of them till the balls ſeparated to a given degree, and then communicated its electricity to the other; and obſerved to what diſtance the balls ſeparated after this communication. It is plain, that if the jars were exactly equal, this would be the diſtance ſought for; as in that caſe the quantity of electricity in the firſt jar would be juſt half as much after the communication as before; but as I could not be ſure that they were exactly equal, I repeated the experiment by electrifying the ſecond jar, communicating its electricity to the firſt, and obſerving how far the balls ſeparated; the mean between theſe two diſtances will evidently be the degree of ſeparation ſought, though the jars were not of the ſame ſize. Having found this, I electrified one row of the battery till the balls ſeparated to the firſt diſtance, and repeatedly communicated its electricity to the plate of coated crown glaſs, taking care to diſcharge the plate each time before the communication was made, till it appeared by the electrometer, that the quantity of electricity in that row was reduced to one half. I found it neceſſary to do this between 11 or 12 times, or $11\frac{1}{4}$ times as I eſtimate it. Whence the quantity of electric fluid in the row may be thus determined.

Let.

Let the quantity in the plate be to that in the row as x to 1; it is plain, that the electricity in the row will be diminished each time it is communicated to the plate, in the proportion of 1 to $1+x$, and consequently after being communicated $11\frac{1}{4}$ times will be reduced in the proportion of 1 to $\overline{1+x}^{11\frac{1}{4}}$; therefore, $\overline{1+x}^{11\frac{1}{4}}=2$; and $1+x=2^{\frac{1}{11\frac{1}{4}}}$. Whence the value of x may easily be found by logarithms. But the readiest way of computing it, and which is exact enough for the purpose, is this: multiply the number of times which you communicated the electricity of the row to the plate, by 1,444; and from the product subtract the fraction $\frac{1}{2}$; the remainder is equal to $\frac{1}{x}$, or the number of times by which the electricity in the row exceeds that in the plate.

The way by which I estimated the strength of the charge given to the battery, was taking a certain number of jars, and electrifying them till the balls of the electrometer separated to a given distance, and then communicating their electricity to the battery. This method proved very convenient; for by using always the same jars, I was sure to give always the same charge with great exactness; and by varying the number and size of the jars, I could vary the charge at pleasure, and besides could estimate pretty nearly the proportion of the different charges to each other. It was also the only convenient method which occurred to me; for I could not have done it conveniently by charging the whole battery till an electrometer suspended from it separated

parated to a given diſtance; becauſe in moſt of the experiments the electricity was ſo weak, that a pair of fine pith balls ſuſpended from the battery would ſeparate only to a very ſmall diſtance; and counting the number of revolutions of the electrical machine is a very fallacious method.

I found, upon trial, that though a ſhock might be procured from this artificial torpedo, while held under water, yet there was too great a diſproportion between its ſtrength, when received this way, and in air; for if I placed one hand on the upper, and the other on the lower ſurface of the electric organs, and gave ſuch a charge to the battery, that the ſhock, when received in air, was as ſtrong as, I believe, that of the real torpedo commonly is; it was but juſt perceptible when received under water. By increaſing the charge, indeed, it became conſiderable; but then this charge would have given a much greater ſhock out of water than the torpedo commonly does. The water uſed in this experiment was of about the ſame degree of ſaltneſs as that of the ſea; that being the natural element of the torpedo, and what Mr. WALSH made his experiments with. It was compoſed of one part of common ſalt diſſolved in 30 of water, which is the proportion of ſalt uſually ſaid to be contained in ſea water. It appeared alſo, on examination, to conduct electricity not ſenſibly better or worſe than ſome ſea water procured from a mineral water warehouſe. It is remarkable, that if I uſed freſh water inſtead of ſalt, the ſhock ſeemed very little weaker, when

received under water than out; which not only confirms what was before ſaid, that ſalt water conducts much better than freſh; but, I think, ſhews, that the human body is alſo a much better conductor than freſh water: for otherwiſe the ſhock muſt have been much weaker when received under freſh water than in air.

As there appeared to be too great a diſproportion between the ſtrength of the ſhock in water and in air, I made another torpedo, exactly like the former, except that the part ABCDE inſtead of wood was made of ſeveral pieces of thick leather, ſuch as is uſed for the ſoles of ſhoes, faſtened one over the other, and cut into the proper ſhape; the pieces of pewter being fixed on the ſurface of this, as they were on the wood, and the whole covered with ſheep ſkin like the other. As the leather, when thoroughly ſoaked with ſalt water, would ſuffer the electricity to paſs through it very freely, I was in hopes that I ſhould find leſs difference between the ſtrength of the ſhock in water and out of it, with this than with the other. For ſuppoſe that in receiving the ſhock of the former torpedo under water, the quantity of electricity which paſſed through the wood and leather of the torpedo, through my body, and through the water, were to each other as T, B, and W; the quantity of electricity which would paſs through my body, when the ſhock was received under water, would be to that which would paſs through it, when the ſhock was received out of water, as $\frac{B}{B+T+W}$ to $\frac{B}{B+T}$; as in the firſt caſe, the quantity

which would paſs through my body would be the $\frac{B}{B+T+W}$ part of the whole; and in the latter the $\frac{B}{B+T}$ part. Suppoſe now, that the latter torpedo conducts N times better than the former; and conſequently, that in receiving its ſhock under water, the quantity of electricity which paſſes through the torpedo, through my body, and through the water, are to each other as NT, B, and W; the quantity of electricity which will now paſs through my body, when the ſhock is received under water, and out of water, will be to each other as $\frac{B}{B+NT+W}$ to $\frac{B}{B+NT}$; which two quantities differ from each other in a leſs proportion than $\frac{B}{B+T+W}$ and $\frac{B}{B+T}$: conſequently, the readier the body of the torpedo conducts, the greater charge will it require to give the ſame ſhock, either in water or out of it; but the leſs will be the difference between the ſtrength of the two ſhocks. It ſhould be obſerved, that this alteration, ſo far from making it leſs reſembling the real torpedo, in all probability makes it more ſo; for I ſee no reaſon to think, that the real torpedo is a worſe conductor of electricity than other animal bodies; and the human body is at leaſt as good, if not a much better conductor than this new torpedo.

The event anſwered my expectation; for it required about three times as great a charge of the battery, to give the ſame ſhock in air, with this new torpedo as with the former; and the difference between its ſtrength when

 received

received under water and out of it, was much lefs than before, and perhaps not greater than in the real torpedo. There is, however, a confiderable difference between the feel of it under water and in air. In air it is felt chiefly in the elbows; whereas, under water, it is felt chiefly in the hands, and the fenfation is fharper and more difagreeable. The fame kind of fhock, only weaker, was felt if, inftead of touching the fides, I held my hands under water at two or three inches diftance from it.

It is remarkable, that I felt a fhock of the fame kind, and nearly of the fame ftrength, if I touched the torpedo under water with only one hand, as with both. Some gentlemen who repeated the experiment with me thought it was rather ftronger. This fhews, that the fhock under water is produced chiefly by the electricity running through one's hand from one part to the other; and that but a fmall part paffes through one's body from one hand to the other. The truth of this will appear with more certainty from the following circumftance; namely, that if I held a piece of metal, a large fpoon for inftance, in each hand, and touched the torpedo with them inftead of my hands, it gave me not the leaft fhock when immerfed in water; though when held in air, it affected me as ftrongly if I touched it with the fpoons as with my hands. On increafing the charge, indeed, its effect became fenfible; and as well as I could judge, the battery required to be charged about twelve times as high to give the fame fhock when the torpedo was touched with the fpoons under water as out of it. It muft be obferved, that in trying this

this experiment, as my hands were out of water, I could be affected only by that part of the fluid which paffed through my body from one hand to the other.

The following experiments were made with the torpedo in air. If I ftood on an electric ftool, and touched either furface of the electric organs with one hand only, I felt a fhock in that hand; but fcarcely fo ftrong as when touching it in the fame manner under water. If I laid a hand on one furface of the electric organs, and with the other touched the tail, I felt a fhock; but much weaker than when touching it in the ufual manner; that is, with one hand on the upper furface of thofe organs, and the other on the lower. If I laid a thumb on either furface of an electric organ, and a finger of the fame hand on any part of the body, except on or very near the fame furface of the organs, I felt a fmall fhock.

In all the foregoing experiments, the battery was charged to the fame degree, except where the contrary is expreffed: they all feem to agree very well with Mr. WALSH's experiments.

Mr. WALSH found, that if he inclofed a torpedo in a flat bafket, open at the top, and immerfed it in water to the depth of three inches, and while the animal was in that fituation, touched its upper furface with an iron bolt held in one hand, while the other hand was dipped into the water at fome diftance, he felt a fhock in both of them. I accordingly tried the fame experiment with the artificial torpedo; and if the battery was charged about fix times as high as ufual, received a fmall fhock in each hand

hand(a). No ſenſible difference could be perceived in the ſtrength, whether the torpedo was incloſed in the baſket or not. The trough in which this experiment was tried was 36 inches long, $14\frac{1}{2}$ broad, and 16 deep; and the diſtance of that hand which was immerſed in the water from the electric organs of the torpedo, was about 14 inches. As it was found neceſſary to charge the battery ſo much higher than uſual, in order to receive a ſhock, it follows, that unleſs the fiſh with which Mr. WALSH tried this experiment were remarkably vigorous, there is ſtill too great a diſproportion between the ſtrength of the ſhock of the artificial torpedo when received under water and out of it. If this is the caſe, the fault might evidently be remedied by making it of ſome ſubſtance which conducts electricity better than leather.

When the torpedo happens to be left on ſhore by the retreat of the tide, it looſens the ſands by flapping its fins, till its whole body, except the ſpiracles, is buried; and it is ſaid to happen ſometimes, that a perſon accidentally treading on it in that ſituation, with naked feet, is thrown down by it. I therefore filled a box, 32 inches long and 22 broad, with ſand, thoroughly ſoaked with ſalt water, to the depth of four inches, and placed the torpedo in it, intirely covered with the ſand, except the upper part of its convex ſurface, and laid one hand on its electrical organs, and the other on the wet ſand about 16 inches from it.

(a) As well as I could judge, the battery required to be charged about 16 or 20 times as high, to give a ſhock of the ſame ſtrength when received this way as when received in the uſual manner with the torpedo out of water.

it. I felt a ſhock, but rather weak; and as well as I could judge, as ſtrong as if the battery had been charged half as high, and the ſhock received in the uſual way.

I next took two thick pieces of that ſort of leather which is uſed for the ſoles of ſhoes, about the ſize of the palm of my hand; and having previouſly prepared them by ſteeping in ſalt water for a week, and then preſſing out as much of the water as would drain off eaſily, repeated the experiment with theſe leathers placed under my hands. The ſhock was weaker than before, and about as ſtrong as if received in the uſual way with the battery charged one-third part as high. As it would have been troubleſome to have trod on the torpedo and ſand, I choſe this way of trying the experiment. The pieces of leather were intended to repreſent ſhoes, and in all probability the ſhoes of perſons who walk much on the wet ſand will conduct electricity as well as theſe leathers. I think it likely, therefore, that a perſon treading in this manner on a torpedo, even with ſhoes on, but more ſo without, may be thrown down, without any extraordinary exertion of the animal's force, conſidering how much the effect of the ſhock would be aided by the ſurprize.

One of the fiſhermen that Mr. WALSH employed aſſured him, that he always knew when he had a torpedo in his net, by the ſhocks he received while the fiſh was at ſeveral feet diſtance; in particular, he ſaid, that in drawing in his nets with one of the largeſt in them, he received a ſhock when the fiſh was at twelve feet

feet diſtance, and two or three more before he got it into his boat. His boat was afloat in the water, and he drew in the nets with both hands. It is likely, that the fiſherman might magnify the diſtance; but, I think, he may ſo far be believed, as that he felt the ſhock before the torpedo was drawn out of water. This is the moſt extraordinary inſtance I know of the power of the torpedo; but I think ſeems not incompatible with the ſuppoſition of its being owing to electricity; for there can be little doubt, but that ſome electricity would paſs through the net to the man's hands, and from thence through his body and the bottom of the boat, which in all probability was thoroughly ſoaked with water, and perhaps leaky, to the water under the boat: the quantity of electric fluid, however, taking this circuit, would moſt likely bear ſo ſmall a proportion to the whole, that this effect can not be accounted for, without ſuppoſing the fiſh to exert at that time a ſurprizingly greater force than what it uſually does.

Hitherto, I think, the effects of this artificial torpedo agree very well with thoſe of the natural one. I now proceed to conſider the circumſtance of the ſhock's not being able to paſs through any ſenſible ſpace of air. In all my experiments on this head, I uſed the firſt torpedo, or that made of wood; for as it is not neceſſary to charge the battery more than one-third part as high to give the ſame ſhock with this as with the other, the experiments were more likely to ſucceed, and the concluſions to be drawn from them would be ſcarcely leſs convincing: for

I find

I find, that five or ſix rows of my battery will give as great a ſhock with the leathern torpedo, as one row electrified to the ſame degree will with the wooden one; conſequently, if with the wooden torpedo and my whole battery, I can give a ſhock of a ſufficient ſtrength, which yet will not paſs through a chain of a given number of links, there can be no doubt, but that, if my battery was five or ſix times as large, I ſhould be able to do the ſame thing with the leathern torpedo.

I covered a piece of ſealing wax on one ſide with a ſlip of tinfoil, and holding it in one hand, touched an electrical organ of the torpedo with the end of it, while my other hand was applied to the oppoſite ſurface of the ſame organ. The ſhock paſſed freely, being conducted by the tinfoil; but if I made, with a penknife, as ſmall a ſeparation in the tinfoil as poſſible, ſo as to be ſure that it was actually ſeparated, the ſhock would not paſs, conformably to what Mr. WALSH obſerved of the torpedo.

I tried the experiment in the ſame manner with the LANE's electrometer deſcribed in p. 202, and found that the ſhock would not paſs, unleſs the knobs were brought ſo near together as to require the aſſiſtance of a magnifying glaſs to be ſure that they did not touch.

I took a chain of ſmall braſs wire, and holding it in one hand, let the loweſt link lie on the upper ſurface of an electric organ, while my other hand was applied to the oppoſite ſurface. The event was, that if the link, held in my hand, was the fifth or ſixth from the bottom, and conſequently, that the electricity had only four or five links to

to paſs through beſides that in my hand, I received a ſhock; ſo that the electricity was able to force its way through four or five intervals of the links, but not more. One gentleman, indeed, found it not to paſs through a ſingle interval; but in all probability the link which lay on the torpedo happened to bear more looſely than uſual againſt that in his hand. If inſtead of this chain I uſed one compoſed of thicker wire, the ſhock would paſs through a great number of links; but I did not count how many. It muſt be obſerved, that the principal reſiſtance to the paſſage of the electrical fluid is formed by the intervals of the lower links of the chain; for as the upper are ſtretched by a greater weight, and therefore preſſed cloſer together, they make leſs reſiſtance. Conſequently the force required to make the ſhock paſs through any number of intervals, is not twice as great as would be neceſſary to make it paſs through half the number. For the ſame reaſon it paſſes eaſier through a chain conſiſting of heavy links than of light ones.

Whenever the electricity paſſed through the chain, a ſmall light was viſible, provided the room was quite dark. This, however, affords no argument for ſuppoſing that the phenomena of the torpedo are not owing to electricity; for its ſhock has never been known to paſs through a chain or any other interruption in the circuit; and conſequently, it is impoſſible that any light ſhould have been ſeen.

In all theſe experiments, the battery was charged to the ſame degree; namely, ſuch that the ſhock was nearly

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of the ſame ſtrength as that of the leathern torpedo, and which I am inclined to think, from my converſation with Mr. WALSH, may be conſidered as about the medium ſtrength of thoſe of a real one of the ſame ſize as this. It was nearly equal to that of the plate of crown glaſs in p. 206. electrified to ſuch a degree as to diſcharge itſelf when the knobs of a LANE's electrometer were at ,0115 inches diſtance; whence a perſon, uſed to electrical experiments, may aſcertain its ſtrength. The way I tried it was by holding the LANE's electrometer in one hand, with the end reſting on the upper ſurface of the plate, and touching the lower ſurface with the other hand, while an aſſiſtant charged the plate by its upper ſide till it diſcharged itſelf through the electrometer and my body. There is, however, a very ſenſible difference between the ſenſation excited by a ſmall jar or plate of glaſs like this, and by a large battery electrified ſo weakly that the ſhock ſhall be of the ſame ſtrength; the former being ſharper and more diſagreeable. Mr. WALSH took notice of this difference; and ſaid, that the artificial torpedo produced juſt the ſame ſenſation as the real one.

As it appeared, that a ſhock of this ſtrength would paſs through a few intervals of the links of the chain, I tried what a ſmaller would do. If the battery was charged only to a fourth or fifth part of its uſual height, the ſhock would not paſs through a ſingle interval; but then it was very weak, even when received through a piece of braſs wire, without any link in it. This chain was quite clean and very little tarniſhed; the loweſt link was larger

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than the reſt, and weighed about eight grains. If I uſed a chain of the ſame kind, the wire of which, though pretty clean, was grown brown by being expoſed to the air, the ſhock would not paſs through a ſingle interval, with the battery charged to about one-third or one-half its uſual ſtrength.

It appears, that in this reſpect the artificial torpedo does not completely imitate the effects of the real one, though it approaches near to it; for the ſhock of the former, when not ſtronger than that of the latter frequently is, will paſs through four or five intervals of the links of a chain; whereas the real torpedo was never known to force his through a ſingle interval. But, I think, this by no means ſhews, that the phenomena of the torpedo are not produced by electricity; but only that the battery I uſed is not large enough. For we may ſafely conclude, from the experiments mentioned in p. 200. and 202. that the greater the battery is, the leſs ſpace of air, or the fewer links of a chain, will a ſhock of a given ſtrength paſs acroſs. For greater certainty, however, I tried, whether if the whole battery and a ſingle row of it were ſucceſſively charged to ſuch a degree, that the ſhock of each ſhould be of the ſame ſtrength when received through the torpedo in the uſual manner, that of the whole battery would be unable to paſs through ſo many links of a chain as that of a ſingle row (b). In order to which I made the following machine.

(b) The battery, as was before ſaid, was divided into ſeven rows, each of which could be uſed ſeparately.

GM,

GM, fig. 5. is a piece of dry wood; F*f*, E*e*, D*d*, C*c*, B*b*, and A*a*, are pieces of braſs wire faſtened to it, and turned up at bottom into the form of a hook, on which is hung a ſmall braſs chain, as in the figure, ſo as to form five loops, each loop conſiſting of five links; the part G is covered with tinfoil, which is made to communicate with the wire A*a*. If I held this piece of wood in one hand, with my thumb on either of the wires F*f*, E*e*, &c. and applied the part G to one ſurface of an electric organ, while with a ſpoon, held in the other hand, I touched the oppoſite ſurface, I received a ſhock, provided the battery was charged high enough, the electricity paſſing through all that part of the chain between A*a*, and my thumb; ſo that I could make the ſhock paſs through more or fewer loops, according to which wire my thumb was placed on; but if the charge was too weak to force a paſſage through the chain, I felt no ſhock, as the wood was too dry to convey any ſenſible quantity of electricity. The event of the experiment was, that if I charged the whole battery to ſuch a degree that the ſhock would but juſt paſs through two loops of the machine, and then charged a ſingle row to ſuch a degree as appeared, on trial, juſt ſufficient to give a ſhock of the ſame ſtrength as the former, it paſſed through all five loops; whether it would have paſſed through more I cannot tell. If, on the other hand, I gave ſuch a charge to the whole battery, and alſo to the ſingle row, as was juſt ſufficient to force a paſſage through two loops of the chain, the ſhock with the whole battery was much ſtronger than that with the ſingle row.

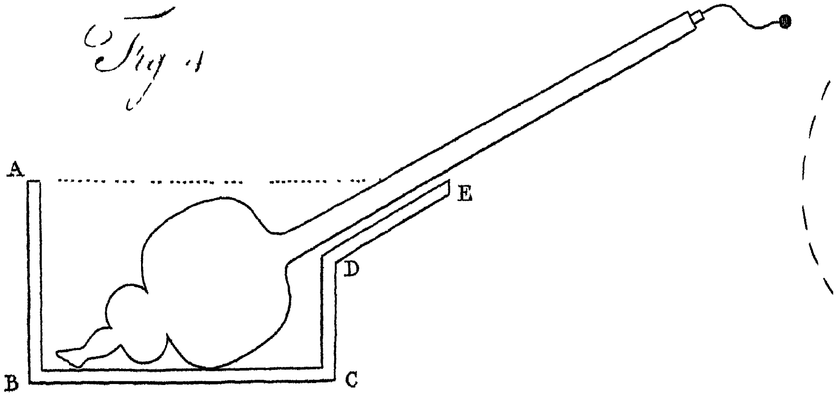
It

It muſt be obſerved, that in the foregoing machine, each loop conſiſted of the ſame number of links, and the links of each loop were ſtretched by the ſame weight; ſo that it required no more force to impell the electricity through one loop than another, which was my reaſon for uſing this machine rather than a plain chain. Conſiderable irregularities occurred in trying the above experiments, and indeed all thoſe with a chain; for it frequently happened, that the ſhock would not paſs with the battery charged to a certain degree, when perhaps a minute after, it would paſs with not more than three-fourths of the charge. The irregularity, however, was not ſo great but that, I think, I may be certain of the truth of the foregoing facts; eſpecially as the experiments were repeated ſeveral times. The uncertainty was at leaſt as great in the experiments with LANE's electrometer, when the knobs were brought ſo cloſe together, as is neceſſary in experiments of this kind.

It appears therefore, that if the whole battery, and a ſingle row of it, are both charged in ſuch a degree as to give a ſhock of the ſame ſtrength, the ſhock with the whole battery will paſs through fewer loops of the chain than that with the ſingle row; ſo that, I think, there can be no doubt, but that if the battery had been large enough, I ſhould have been able to give a ſhock of the uſual ſtrength, which yet would not have paſſed through a ſingle interval of the links of a chain.

On the whole, I think, there ſeems nothing in the phenomena of the torpedo at all incompatible with electricity;

Fig 4



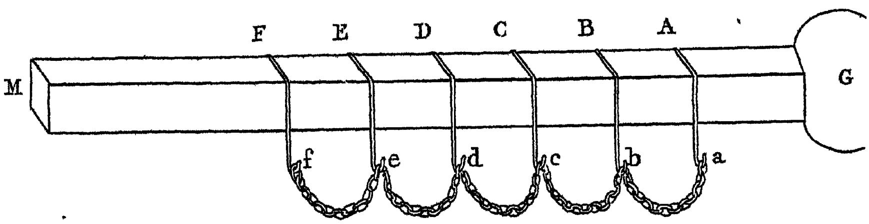
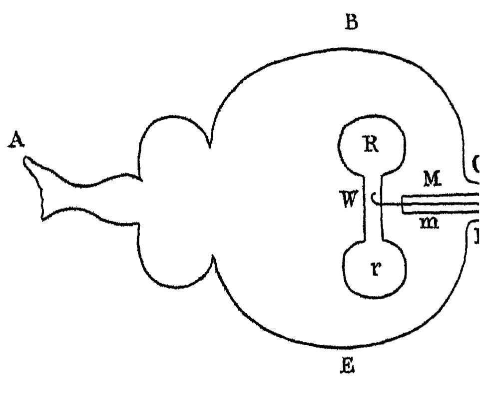


Fig. 5.



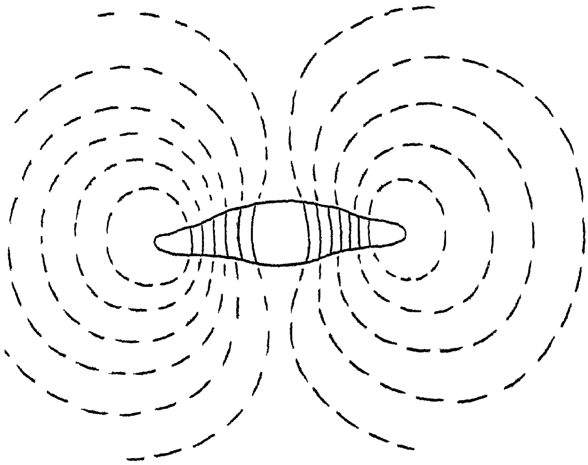


Fig 1

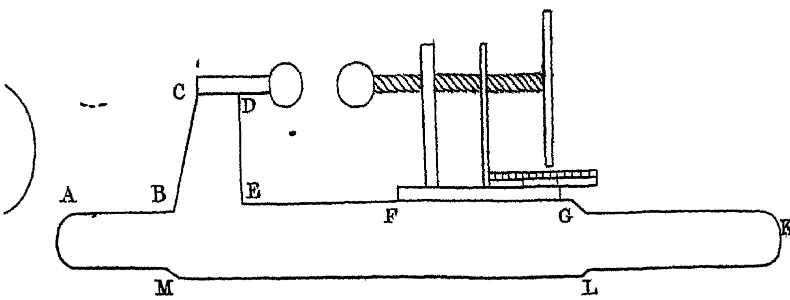


Fig 2

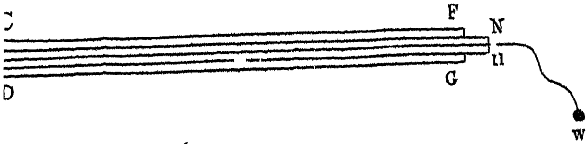


Fig 3

tricity; but to make a compleat imitation of them, would require a battery much larger than mine. It may be asked, where can such a battery be placed within the torpedo? I answer, perhaps it is not necessary that there should be any thing analogous to a battery within it. The case is this; it appears, that the quantity of electric fluid, transferred from one side of the torpedo to the other, must be extremely great; for otherwise it could not give a shock, considering that the force with which it is impelled is so small as not to make it pass through any sensible space of air. Now if such a quantity of fluid was to be transferred at once from one side to the other, the force with which it would endeavour to escape would be extremely great, and sufficient to make it dart through the air to a great distance, unless there was something within it analogous to a very large battery. But if we suppose, that the fluid is gradually transferred through the electrical organs, from one side to the other, at the same time that it is returning back over the surface, and through the substance, of the rest of the body; so that the quantity of fluid on either side is during the whole time very little greater or less than what is naturally contained in it; then it is possible, that a very great quantity of fluid may be transferred from one side to the other, and yet the force with which it is impelled be not sufficient to force it through a single interval of the links of a chain. There seems, however, to be room in the fish for a battery of a sufficient size; for Mr. HUNTER has shewn, that each of the prismatical columns of which

which the electrical organ is compofed, is divided into a great number of partitions by fine membranes, the thicknefs of each partition being about the 150th part of an inch; but the thicknefs of the membranes which form them is, as he informs me, much lefs. The bulk of the two organs together in a fifh $10\frac{1}{3}$ inches broad, that is of the fame fize as the artificial torpedos, feems to be about $24\frac{1}{2}$ cubic inches; and therefore the fum of the areas of all the partitions is about 3700 fquare inches. Now 3700 fquare inches of coated glafs $\frac{1}{150}$ of an inch thick will receive as much electricity as 30500 fquare inches ,055 of an inch thick[(a)]; that is, 305 times as much as as the plate of crown glafs mentioned in p. 206, or about $2\frac{3}{4}$ times as much as my battery, fuppofing both to be electrified by the fame conductor; and if the glafs is five times as thin, which perhaps is not thinner than the membranes which form the partitions, it will contain five times as much electricity, or near fourteen times as my battery.

It was found, both by Dr. WILLIAMSON and by a committee appointed by the Philofophical Society of Penfylvania, that the fhock of the *Gymnotus* would fometimes pafs through a chain, though they never perceived any light. I therefore took the fame chain which I ufed in the foregoing experiments, confifting of 25 links, and fufpended it by its extremities from the extreme hooks of the machine defcribed in p. 221, and applying the end of the machine to the negative fide of the battery,

(a) Vide Note in p. 206.

touched

touched the poſitive ſide with a piece of metal held in the other hand, ſo as to receive the ſhock through the chain without its paſſing through the torpedo; the battery being charged to ſuch a degree that the ſhock was conſiderably ſtronger than what I uſually felt in the foregoing experiments. I found that if the chain was not ſtretched by any additional weight, the ſhock did not paſs at all: If it was ſtretched by hanging a weight of ſeven pennyweights to the middle link, it paſſed, and a light was viſible between ſome of the links; but if fourteen pennyweights were hung on, the ſhock paſſed without my being able to perceive the leaſt light, though the room was quite dark; the experiment being tried at night, and the candle removed before the battery was diſcharged. It appears, therefore, that if in the experiments made by theſe gentlemen the ſhock never paſſed, except when the chain was ſomewhat tenſe, which in all probability was the caſe, the circumſtance of their not having perceived any light is by no means repugnant to the ſuppoſition that the ſhock is produced by electricity.

XIII. *Ob-*

XIII. *Obſervations on Reſpiration, and the Uſe of the Blood.* *By* Joſeph Prieſtley, *LL.D. F. R. S.*

R. Jan. 25, 1776.

THERE is, perhaps, no ſubject in phyſiology, and very few in philoſophy in general, that has engaged more attention than that of the uſe of *reſpiration*. It is evident, that without breathing moſt animals would preſently die; and it is alſo well known, that the ſame air will not long anſwer the purpoſe: for if it has been frequently reſpired, the breathing of it is as fatal as the total deprivation of air. But by what property it is, that air contributes to the ſupport of animal life; and why air that has been much breathed will no more anſwer the purpoſe, ſeems not to have been diſcovered by any of the many philoſophers and phyſicians who have profeſſedly written upon the ſubject; and it might have continued to elude all *direct inveſtigation*, when it diſcovered itſelf, without any trouble or thought, in the courſe of my reſearches into the properties of different kinds of air, which had at firſt quite another object.

In theſe experiments it clearly appeared, that reſpiration is a *phlogiſtic proceſs*, affecting air in the very ſame manner

as every other phlogiſtic proceſs (*viz.* putrefaction, the efferveſcence of iron-filings and brimſtone, or the calcination of metals, &c.) affects it; diminiſhing the quantity of it in a certain proportion, leſſening its ſpecific gravity, and rendering it unfit for reſpiration or inflammation, but leaving it in a ſtate capable of being reſtored to a tolerable degree of purity by agitation in water, &c. Having diſcovered this, I concluded, as may be ſeen Phil. Tranſ. vol. LXII. p. 187. and *Obſervations upon Air*, vol. I. p. 78. 277. that the uſe of the lungs is to carry off a putrid *effluvium*, or to diſcharge that phlogiſton, which had been taken into the ſyſtem with the aliment, and was become, as it were, *effete*; the air that is reſpired ſerving as a *menſtruum* for that purpoſe.

What I then concluded to be the uſe of *reſpiration* in general, I have now, I think, proved to be effected by means of the *blood*, in conſequence of its coming ſo nearly into contact with the air in the lungs; the blood appearing to be a fluid wonderfully formed to imbibe, and part with, that principle which the chemiſts call phlogiſton, and changing its colour in conſequence of being charged with it, or being freed from it; and affecting air in the very ſame manner, both out of the body and in the lungs; and even notwithſtanding the interpoſition of various ſubſtances, which prevent its coming into immediate contact with the air.

As it may not be unpleaſing or unuſeful, I ſhall, before I relate my own experiments, briefly recite the principal of the opinions which have been held con-

cerning the uſe of reſpiration, from HALLER's excellent *Syſtem of Phyſiology*, and ſome others of the moſt eminent writers upon that ſubject.

HIPPOCRATES reckoned air among the *aliments* of the body. But it was more generally the opinion of the ancients, that, there being a kind of *vital fire* kept up in the heart, the heat of the blood was tempered in the lungs. GALEN alſo ſuppoſed, that there was ſomething equivalent to a fire conſtantly kept up in the heart; and that the chief uſe of the lungs was to carry off ſuch vapours as were equivalent to ſmoke thrown off from that fire. HALLER, vol. III. p. 354. CARTESIUS maintained the ſame vital fire in the heart, ſuppoſing that air was neceſſary for cooling and condenſing the blood. *Ibid.* p. 343.

Of the more modern phyſiologiſts, ſome have thought that the air itſelf is taken into the lungs; others, that it is only ſomething extracted from the air, as the more ſubtle parts of that fluid, an ether, or aerial nitre; while others ſuppoſe it to be the air itſelf, but diſſolved in water, and therefore in an unelaſtic ſtate, *ibid.* p. 321.

Moſt of thoſe who think that air is taken into the blood ſuppoſe it to be taken in by the lungs, *ibid.* p. 330. Some ſuppoſe, that the effect of the admiſſion of this air into the blood is a fermentation, p. 332. Others ſuppoſe, that it acts by its ſpring, preventing the too cloſe contact of the globules, and thereby preſerving its fluidity, inteſtine motion, and heat, *ibid.* BERTIER ſuppoſed, that the circulation of the blood was, in a great meaſure, owing to the admiſſion of air into it. VAN HELMONT aſcribed the volatility of the fixed ele-

ments

ments in the food to this air, p. 336.; and STEVENSON thought, that the air which had circulated in the blood, and which had heated the blood too much, was exhaled by the lungs, p. 355.

Others say, that the air itself is not admitted into the blood, but only some active, spirituous, and ethereal particles; that this vital spirit passes from the lungs to the heart and arteries, and at length becomes the animal spirits, which are by this means generated from the air, p. 333. Others, who do not admit that the animal spirits are derived from the air, still say that some other *vital principle* comes from thence. This vital principle MALPIGHIUS supposes to be a saline vapour; LISTER, a hot, inflammable, sulphureous spirit; VIEUSSENIUS, a volatile acid salt, which keeps up the fermentation of the blood; and BRYAN ROBINSON, the aerial acid, which preserves the blood from putrefaction; preserves also its density, and strengthens the animal fibres. For this reason he supposes it is that we feel ourselves refreshed in cold air, as it abounds with a more plentiful acid quality, p. 334. They who suppose that nitre is taken from the air into the blood, ascribe to that principle its fermentation, its heat, and its density, p. 334.

It is a received opinion, that one use of the lungs is to attenuate the blood, p. 359; and MALPIGHIUS adds, that by this means, the different particles of the blood become thoroughly mixed together; while others think that the blood is condensed in the lungs; and others, that the globules, and all the finer humours, receive their configuration there, *ibid.* Some, without considering the air

air as of any other uſe than to put the lungs in motion, think, that heat is produced in the lungs by the attrition of the blood in paſſing through them. *Miſc. Taurin.* vol. V. p. 36. The red colour of the blood has been thought by ſome to be cauſed by this attrition in the lungs; but LOWER refuted this notion, chiefly by obſerving, that the attrition of the blood is greater in the muſcles, from which, however, it always returns black, *Ibid.* vol. I. p. 74.

Dr. WHYTT thought there was ſomething of a vital and ſtimulating nature derived from the air into the blood, by means of which it made the heart to contract, HALLER, vol. III. p. 336.

BOERHAAVE ſays, that air not changed is deadly; not on account of heat, rarefaction, or denſity, but for ſome other occult cauſe. *Miſc. Taurin.* vol. V. p. 30.

Dr. HALES, who has thrown much more light upon the doctrine of air than all his predeceſſors, was equally ignorant of the uſe of it in reſpiration; and at different times ſeems to have adopted different opinions concerning it.

In his *Statical Eſſays*, vol. II. p. 321. he ſuppoſes, that air is rendered alcaline by breathing, and corrected, in ſome meaſure, by the fumes of vinegar.

In agreement, as he obſerves, with BOERHAAVE, he ſays, p. 100. that the blood acquires its warmth chiefly in the lungs, where it moves with much greater rapidity than in any other capillary veſſels of the body, vol. II. p. 87; but that one uſe of the air is to cool the blood, p. 94; and he makes an eſtimate of the degree of this refrigeration. The red colour of the globules of blood, he ſays, p. 88,

intimates

intimates their abounding with ſulphur, which makes them more ſuſceptible and retentive of heat than thoſe bodies which have leſs of it.

He alſo ſuppoſes, p. 102, that another great uſe of the lungs is to attenuate and ſeparate the globules of blood; and that the floridneſs of the arterial blood above the venal may, in a good meaſure, be owing to the ſtrong agitation, friction, and comminution, which it undergoes in paſſing through them. In like manner, in an experiment which he made for the purpoſe, blood much agitated in a cloſe glaſs veſſel was obſerved to be very florid, not only on its ſurface, but through its whole ſubſtance, as arterial blood is, vol. II. p. 102. I would obſerve, however, that in this expement, the blood muſt have acquired its florid colour from the air with which it was agitated.

He adds, that it is probable, that the blood may, in the lungs, receive ſome other important influence from the air, which is in ſuch great quantities inſpired into them. In other places, however, he explodes the doctrine of a *vivifying ſpirit* in the air. It has long, he ſays, been the ſubject of inquiry to many, to find of what uſe it is in reſpiration; which, though it may in ſome reſpects be known, yet it muſt be confeſſed, that we are ſtill much in the dark about it, vol. II. p. 102.

Suffocation, he ſays, vol. II. p. 271. conſiſts chiefly in the falling flat of the lungs, occaſioned by the groſſneſs of the particles of a thick noxious air, they being, in that floating ſtate, moſt eaſily attracted by each other, as we find that ſulphur, and the elaſtic repelling particles of air are;

4 and

and conſequently unelaſtic, ſulphureous, ſaline, and other floating particles, will moſt eaſily coaleſce, whereby they are rendered too groſs to enter the minute veſicles, which are alſo much contracted, as well by the loſs of the elaſticity of the confined air, as by the contraction occaſioned by the ſtimulating acid ſulphureous vapours. And hence it is not improbable, that one great deſign of nature in the ſtructure of this important and wonderful *viſcus*, was to frame the veſicles ſo very minute, thereby effectually to hinder the ingreſs of groſs, feculent particles, which might be injurious to the animal economy.

Laſtly, he concludes, that the effect of reſpiration is to abate, and in part deſtroy, the elaſticity of the air; and as this was effected by ſuphureous vapours, and he could breathe for a longer time air that had paſſed through cloaths dipped in a ſolution of ſalt of tartar, he concluded, that the air had been mended by the tartar having ſtrongly imbibed the ſulphureous, acid, and watery vapours, vol. I. p. 267.

HALLER, after reciting the opinions of all that had gone before him, ſuppoſes, with Dr. HALES, that, in conſequence of the air loſing its ſpring in the lungs, they cannot be kept dilated; and therefore, they muſt collapſe, and the circulation of the blood be impeded, vol. III. p. 258. When he ſtates his opinion concerning the uſe of the lungs more fully, he ſays, that the true uſe of them is partly inhaling, and partly exhaling, p. 351. That the lungs inhale both water and air; but that in the lungs the air loſes its elaſtic property, ſo as to be eaſily ſoluble in water or vapour, p. 352.: and he thinks it probable,

probable, that this air ferves as a cement to bind the earthy parts together. He alfo makes no doubt, but that various other matters, mifcible with water, are inhaled by the lungs; and he even thinks it not improbable, that the air may carry fome electric virtue along with it. The principal exhalation of the lungs, he thinks, to be water, abounding with oily, volatile, and faline principles; and thefe oily and fetid vapours, he thinks, to be the *fuligines* of GALEN and other ancients, p. 354.

Mr. CIGNA of Turin, has given much attention to this curious fubject, as appears by two Memoirs of his; one in the firft volume of the *Mifcellanea Taurinenfia*, in which he very well accounts for the florid red colour of the blood; and the other, which is a much more elaborate Memoir, intitled, *De Refpiratione*, in the fifth volume of the fame work, juft publifhed, or about to be publifhed, the copy of the article having been fent to me by the author.

He takes it for granted, that air which has once been breathed is unfit for farther refpiration, on no other account than its being loaded with *noxious vapours*, which difcover themfelves by a fetid fmell. *Mifc. Taurin.* vol. V. p. 30. And he takes it for granted, that the elafticity of air is diminifhed by refpiration, though he does not confider that diminution of elafticity as the caufe of its noxious quality. He therefore concludes, that air which has been breathed, fuffocates by means of the irritation which it occafions to the lungs, by which the bronchia, and the lungs themfelves, are contracted, fo as to refift the entrance of the air; and therefore, that refpired air is

 noxious

noxious on the ſame account as mephitic vapours, or thoſe of burning brimſtone, p. 31; that, in frequently breathing the ſame air, it becomes ſo loaded with theſe vapours, as to excite a convulſion in the lungs, and thereby render them unfit for tranſmitting the blood, p. 42.

This philoſopher ſuppoſes that air enters the pores of the blood, retaining its elaſtic power, p. 50. and that it continues at reſt there, becauſe its endeavour to eſcape is counteracted by the equal preſſure of the ambient medium, p. 52. This air, he ſuppoſes to be introduced into the blood by the chyle, and never by the way of the lungs, except when, by ſome means or other, the equilibrium between the air in the blood and the external air is loſt, p. 57. If the external air be rarer than the internal, the air in the blood, expanding itſelf, will inflate the animal, and have the ſame effect as air introduced into the veins.

What we are chiefly indebted to M. CIGNA for, is his deciſive experiments with reſpect to the florid colour of the blood, which he clearly proves to be cauſed by the contact of air; though he afterwards ſeems willing to deſert that hypotheſis. It was often imagined, that the reaſon why the lower part of a quantity of blood was black, while the ſurface was red, was, that the black particles, being heavier than the reſt, ſubſided to the bottom; but this opinion our author clearly refutes. He found, that when he put a little oil upon a quantity of blood, it remained black throughout; but that when he took away the red part, and expoſed to the air the lower

laminæ,

laminæ, which were black, they also became successively red, till the whole mass acquired that colour, *Misc. Taurin.* vol. I. p. 73. Also, at the request of M. CIGNA, Father BECCARIA tried what would be the effect of exposing blood in *vacuo*; and he found, that in those circumstances, it always continued black; but that, by exposing it again to the air, it became red, p. 68.

M. CIGNA concludes his first dissertation with observing, that it is not easy to say how it comes to pass, that the lower part of a mass of blood becomes black, whether by the air which it had imbibed escaping from it, or by its depositing something saline, necessary to contribute to its redness, or by the pressure of the atmosphere; but he inclines to think, that air mixed with blood, and interposed between the globules, preserves its redness: but that by concreting it is expelled from it, or becomes so fixed as to be incapable of making it red. This opinion, he thinks, is rendered in some measure probable, by the increased density of concreted blood, and by the emission of air from other fluids in a concrescent state, p. 74.

Notwithstanding what he had advanced in his first Memoir, yet in the second, which was written several years after it, he doubts whether the change of colour in the blood takes place in the lungs; but if it does, he inclines to ascribe this effect to the *evaporation* from the blood in the lungs: and though he always found, that the colour of the blood was changed by the contact of air, yet when he considered that evaporation must, as he thought, necessarily attend the contact of air, he imagined, that this

effect might equally be attributed to this circumstance. But he acknowledges, that this hypothesis ought not to be received till it be confirmed by experiments, *Misc. Taurin.* vol. V. p. 61.

Upon the whole, he concludes, that the principal use of air to the *blood*, is to preserve the equilibrium with the external air, and to prevent the vessels from being rendered unfit to transmit the blood, on account of the external pressure; whereas, by means of the air they contain, the fluids move in their proper vessels as freely as in *vacuo*, and the membranes and viscera also easily slide over each other, p. 63. And with respect to the use of the *lungs*, since he imagined that air is not introduced into the blood by means of them, he thinks, that because such lungs as those of man are given to the warmer animals only, the chief use of respiration is exhalation, and consequently the cooling of the blood, p. 65.

The last writer whom I shall quote upon this subject, is the late ingenious Mr. HEWSON; who says, in his *Experimental Inquiry into the Properties of Blood*, p. 9. "As the colour of the blood is changed by air out of the "body, it is presumed, that the air in the lungs is the "immediate cause of the same change in the body." That this change is really produced in the lungs, he is persuaded, he says, from experiments, in which he distinctly saw the blood of a more florid red in the left auricle of the heart than it was in the right; but how this effect is produced, he says, is not yet determined.

Since

Since fome of the neutral falts, and particularly nitre, has a fimilar effect on the colour of the blood; fome, fays he, attribute this difference to the nitre abforbed from the air, while in the lungs. But this, he adds, is a mere hypothefis, for air contains no nitre, and moft of the neutral falts produce the fame effect in fome degree.

Since, however, a folution of nitre does produce this effect upon blood, inftantly making the very blackeft of it of a beautiful florid red, though this effect is not peculiar to nitre (for a folution of common falt does nearly the fame thing) I own I am inclined to afcribe this effect to the air; efpecially fince I have proved, as I apprehend, that atmofpherical air confifts of earth and fpirit of nitre. Poffibly, therefore, the air we breathe may be fo far decompofed, as to communicate fomething of nitre to the blood, in its paffage through the lungs.

After this review of the obfervations and opinions of others on this important queftion in phyfiology, I fhall proceed to recite my own. It may appear fomething extraordinary, that among fuch a variety of opinions concerning the ufe of refpiration, the right one fhould never have been fo much as conjectured, though unfupported by the proper proof. But indeed, this animal function, and the phlogiftic proceffes in chemiftry, efpecially that of the calcination of metals, which is, perhaps, the moft fimple of them, are to appearance very different things; and therefore, it is the lefs to be wondered, that no perfon

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ſhould have imagined, they would produce the ſame effect on the air in which they were performed.

That reſpiration, however, is, in reality, a true phlogiſtic proceſs, cannot, I think, admit of a doubt, after its being found, that the air which has ſerved for this purpoſe is left in preciſely the ſame ſtate as that which has been expoſed to any other phlogiſtic proceſs. And ſince all the blood in the body paſſes through the lungs, and, according to Mr. HEWSON's obſervations and others, the remarkable change between the colour of the venal and arterial blood takes place there, it can hardly be doubted, that it is by means of the *blood* that the air becomes phlogiſticated in paſſing through the lungs; and therefore, that one great uſe of the blood muſt be to diſcharge the phlogiſton with which the animal ſyſtem abounds, imbibing it in the courſe of its circulation, and imparting it to the air, with which it is nearly brought into contact, in the lungs; the air thus acting as the great menſtruum for this purpoſe.

Though I had no doubt concerning this concluſion from my former experiments, I thought ſo great a problem deſerved as much illuſtration as could be given to it; and therefore I was willing to try, whether the blood was of ſuch a nature, as to retain any of this power of affecting air when congealed, and out of the body, that it has when it is fluid, and in the body; and the experiments have fully anſwered my expectations.

Having taken the blood of a ſheep, and let it ſtand till it was coagulated, and the ſerum was ſeparated from it (after which the ſurface, being expoſed to the common

air, is well known to assume a florid red colour, while the inside is of a much darker red, bordering upon black) I introduced pieces of the crassamentum, contained in nets of open gauze, or of wire, sometimes through water, and sometimes through quicksilver, into different kinds of air, and always found that the blackest parts assumed a florid red colour in common air, and more especially in dephlogisticated air, which is purer and more fit for respiration than common air (and accordingly the blood always acquired a more florid colour, and the change was produced in less time in this than in common air) whereas the brightest red blood became presently black in any kind of air that was unfit for respiration, as in fixed air, inflammable air, nitrous air, or phlogisticated air; and after becoming black in the last of these kinds of air, it regained its red colour upon being again exposed to common air, or dephlogisticated air; the same pieces becomming alternately black and red, by being transferred from phlogisticated to dephlogisticated air; and *vice versâ*.

In these experiments the blood must have parted with its phlogiston to the common air, or dephlogisticated air, and have imbibed it, and have become saturated with it, when exposed to phlogisticated, nitrous, inflammable, or fixed air. The only difficulty is with respect to the fixed air; for all the other kinds certainly contain phlogiston. But, as I have observed in the account of my experiments on vitriolic acid air, phlogiston seems to be necessary to the constitution of every kind of air; and besides, the blackness of the blood may arise from other causes

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than its acquiring phlogiſton. GABER, for inſtance, obſerves, that blood becomes black when it begins to putrify, as it does alſo whenever it is dried and hardened near the fire. Father BECCARIA alſo found, as I have obſerved, that red blood *continued* (and he could hardly fail to obſerve alſo, that it *became*) black in *vacuo*, where it could not have imbibed phlogiſton. This I found to be the caſe when the blood was covered two inches and a half with ſerum; but it regained its florid colour when it was expoſed to the open air.

In general, however, it cannot be expected, that when blood has become black without having received phlogiſton *ab extra*, it will recover its florid colour by being expoſed to the air. For the delicacy of its texture, and conſequently its capacity of being eaſily affected by phlogiſton, may be eſſentially altered by internal cauſes of blackneſs. This is even the caſe when blood has become black by being expoſed to nitrous and inflammable air, though this change is probably effected by its imbibing phlogiſton.

I expoſed pieces of the ſame maſs of red blood to theſe two kinds of air, and alſo to fixed air at the ſame time. They all became black; but that which was in the inflammable air was the leaſt ſo, and none of them recovered their florid colour in the open air. But at another time, a piece of craſſamentum, which had become black in fixed air, did, in ſome meaſure, and very ſlowly, recover its florid colour in dephlogiſticated air. Perhaps the pieces that had loſt their colour in the nitrous and inflam-

inflammable air might have recovered it by means of this more powerful menstruum.

Since, however, blood, after becoming black in phlogisticated air, is always capable of resuming its red colour on being again exposed to pure air, it may be concluded, that the preceding blackness, discharged in the pure air, and producing the constant effect of phlogiston, in depraving the air, was owing to the phlogiston it had imbibed in the former situation, and which it parted with in the latter. And this is remarkably the case when blood is transferred from phlogisticated into dephlogisticated air. Even the circumstance of the *deeper colour* is sufficient to give a chemist a suspicion that it contains more phlogiston than blood of a lighter colour.

When I had found how readily pieces of blood changed their colour, according to the quality of the air to which they were exposed, I proceeded to examine the state of that air, in order to observe what change had taken place in it; and as dephlogisticated air admits of a more sensible change of quality than common air, I gave it the preference in this experiment; putting a piece of crassamentum, about the bigness of a walnut, into the quantity of about five ounce measures of this air.

This process I continued for the space of twenty-four hours, changing the blood about ten or twelve times; after which I found the air so far depraved, that whereas, at the beginning of the experiment, one measure of it and two of nitrous air occupied the space of no more than half a measure, the same mixtures afterwards occupied

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the ſpace of a meaſure and a half. Now ſince air is univerſally depraved by phlogiſton, and in this ſenſe, I believe, by nothing elſe, it is evident, that this black blood muſt have communicated phlogiſton to the air; and conſequently its change of colour from black to a florid red muſt have been occaſioned by the ſeparation of phlogiſton from it.

The next day, when, of courſe, the blood was nearer to a ſtate of putrefaction, in which every kind of ſubſtance, without exception, will injure reſpirable air, I put a quantity of red blood, tinged in a few places with black, which I could not eaſily ſeparate from it, to about the ſame quantity of the ſame dephlogiſticated air, and ſuffered it to ſtand, without changing, for the ſame ſpace of time; when it was ſo little injured, that the meaſures abovementioned occupied the ſpace of only two-thirds of a meaſure.

That blood has a power of taking phlogiſton from air, as well as imparting phlogiſton to air, I ſatisfied myſelf by expoſing blood of a very beautiful florid colour to nitrous air, inflammable air, and phlogiſticated air. The two firſt mentioned kinds of air were conſiderably diminiſhed by the proceſs, which was continued two days, during which time the blood had been changed five or ſix times.

The nitrous air, by this means, loſt a great proportion of its power of diminiſhing, that is, phlogiſticating, common air. For now two meaſures of common air and one of this occupied the ſpace of $2\frac{1}{4}$, inſtead of $1\frac{3}{4}$ meaſures. The inflammable air, though ſtill inflammable,

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was rendered in ſome degree wholeſome by the proceſs; being, after this, conſiderably diminiſhed by nitrous air, which is a ſtate to which it is brought by agitation in water, and which, continued longer, deprives it of its inflammability likewiſe. It cannot be doubted, therefore, but that, in both theſe caſes, the red blood, by becoming black, received phlogiſton from theſe two kinds of air.

With reſpect to the phlogiſticated air, I only obſerved that, after a few hours expoſure to the red blood, it was ſenſibly, but not much, diminiſhed by nitrous air, which otherwiſe it would not have been in the leaſt degree. This blood, however, was of the lighteſt colour; that is, according to my hypotheſis, the moſt free from phlogiſton, of any that I have ever ſeen; and I have tried the ſame thing, without ſucceſs, with blood of a leſs florid colour, though as florid as the common air could make it. But it ſhould be conſidered, that the proper function of the blood is not to receive phlogiſton from *air*, not meeting with any phlogiſticated air in the courſe of its circulation, but to communicate phlogiſton to air; and therefore, there is by no means the ſame reaſon to expect, that air will be mended by red blood, as that it will be injured by black blood.

It may be objected to this hypotheſis, concerning the uſe of the blood, that it never comes into actual contact with the air in the lungs, but is ſeparated from it, though as Dr. HALES ſtates it, at the diſtance of no more than a thouſandth part of an inch. The red globules alſo ſwim

in a large quantity of ſerum, which is a fluid of a quite different nature.

In order to aſcertain the effect of theſe circumſtances, I took a large quantity of black blood, and put it into a bladder moiſtened with a little ſerum, and tying it very cloſe, hung it in a free expoſure to the air, though in a quieſcent ſtate; and the next day I found, upon examination, that all the lower ſurface of the blood, which had been ſeparated from the common air by the intervention of the bladder (which is an animal membrane, ſimilar to that which conſtitutes the veſicles of the lungs, and is at leaſt as thick) and likewiſe a little ſerum, had acquired a coating of a florid red colour, and as thick, I believe, as it would have acquired, if it had been immediately expoſed to the open air; ſo that this membrane had been no impediment to the action of the air upon the blood. In this caſe it is evident to obſerve, that the change of colour could not be owing to *evaporation*, as Mr. CIGNA conjectures. This experiment I repeated, without previouſly moiſtening the bladder, and with the very ſame reſult.

I obſerved alſo, that when I cut out a piece of the craſſamentum, and left the remainder in the veſſel with the *ſerum*, not only that part of the ſurface which was expoſed to the air, but that which was ſurrounded with *ſerum*, and even covered with it to the depth of ſeveral inches, acquired the florid colour; ſo that this deep covering of ſerum, which muſt have effectually prevented all evaporation, was no more an impediment to the mutual action of the blood and the air, than the bladder had been.

been. The ferum of the blood, therefore, appears to be as wonderfully adapted to anfwer its purpofe, of a vehicle for the red globules, as the red globules themfelves: for the flighteft covering of water, or *faliva*, effectually prevents the blood from acquiring its florid colour; and Mr. CIGNA found that this was the cafe when it was covered with oil.

That it is really the air, acting through the ferum, and not the ferum itfelf, that gives the florid colour to the blood, is clearly afcertained by the following experiment. I took two equal portions of black blood, and put them into equal cups, containing equal quantities of ferum, which covered them to the depth of half an inch. One of thefe cups ftanding in the open air, and the other being placed under an exhaufted receiver, the former prefently acquired a florid colour, while the other continued twelve hours as black as at firft. Being taken out of the receiver, it ftood all night in the open air without becoming red, and continued black ever after, even when the ferum was poured off.

I alfo more completely fatisfied myfelf of the influence of the air upon the blood, through a body of ferum, by the reverfe of this experiment. For I found that red blood became black through the depth of two inches of ferum, when the veffel containing it was expofed to phlogifticated air; fo that the red globules of the blood both receive, and part with phlogifton by means of the air, notwithftanding the interpofition of a large body of the fluid in which they naturally float.

Except

Except ſerum, *milk* is the only animal fluid that I have tried, through which the air can act upon blood: for black blood became red when it was plunged in milk, in the ſame manner as if it had been covered with ſerum. In urine, indeed, black blood becomes inſtantly red; but this is not owing to the action of the air, through the urine, but to the ſaline nature of that fluid.

In ſome caſes, care muſt be taken to diſtinguiſh the floridneſs with which ſome detached parts of a quantity of blood are tinged, from that which penetrates the ſolid parts of it. In *ſaliva*, and in water impregnated with alkaline ſalt, fixed or volatile, and alſo in ſpirit of wine, the extreme angles and edges of pieces of craſſamentum and ſmall detached parts, floating in thoſe liquors, will appear of a very florid red, while the compact maſs of blood continues dark. The florid colour of the prominent and detached parts, in theſe caſes, ſeems to be the mere effect of the minute diviſion of the parts of the craſſamentum in the fluid in which thoſe parts float; when at the ſame time it has no ſuch effect on thoſe parts which remain compact, nor has the air the leaſt power of acting on the blood through the liquor.

I had imagined, that ſince black blood contains more phlogiſton than red blood, that difference would have appeared in the *air* produced from them, either by being ſimply diſſolved in ſpirit of nitre, or when dried and made into a paſte with that acid. But the difference was too ſmall to be ſenſible to this kind of teſt. For this purpoſe, however, I had ſome blood drawn from the vein

of

of a sheep, and also took some that came first after killing it, as the butchers usually do, by dividing the *carotid* artery; but though I dissolved the black part of the former, and the red part of the latter, in equal quantities of the same spirit of nitre, I found no sensible difference in the air that they yielded. The air that I got from them when dried, and made into a paste with spirit of nitre, was likewise equally indistinguishable. The quantity of air from this process was very great, and was produced irregularly, as I have observed it to have been when produced by a solution in spirit of nitre without drying. *Observations on Air*, vol. II. p. 155. Half of this produce was fixed air, and the rest phlogisticated, except that a candle burned in it with a lambent blue flame. It is evident, however, from this experiment, that even the most florid blood contains a considerable quantity of phlogiston; for, otherwise, this air would have been dephlogisticated.

I would conclude this paper with observing, that I have found a very great difference in the constitution of blood with respect to its property of being affected by the influence of the air; some becoming very soon of a light florid colour, and the *stratum* of this colour soon growing very thick; whereas, in other cases, the colour of the blood, in the most favourable circumstances, has continued much darker, and the lighter colour has never penetrated very far.

As the principal use of the blood seems to be its power of receiving and discharging phlogiston, and the degree

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in which it poſſeſſes this power is eaſily aſcertained by the eye, it might not, perhaps, be unworthy of being particularly attended to by phyſicians. To eſtimate the goodneſs of blood, according to this criterion, nothing is requiſite but to obſerve the lightneſs of the colour, and the depth of the light-coloured *ſtratum*, after it has been expoſed to the air for a given time. In caſes in which the blood is unuſually black, and but little affected by common air, it ſhould ſeem, that breathing a purer air might be preſcribed with advantage.

In general, the blood that I have been able to procure in the city has not been ſo good as that which I have got in the country; owing, perhaps, to the cattle having been much driven, and heated before they were killed.

XIV. *Experiments on Water obtained from the melted Ice of Sea-Water, to ascertain whether it be fresh or not; and to determine its specific Gravity with respect to other Water. Also experiments to find the Degree of Cold in which Sea-Water begins to freeze. By Mr.* Edward Nairne. *Addressed to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Hampstead, Feb. 1, 1776.

SIR,

R. Feb. 1, 1776. IT having been suggested, in a conversation at which I was present, that the ice of sea-water is not fresh; and that if the ice found near the poles be really so, it must probably be the ice of fresh water discharged into the sea from large rivers in those parts: I thought the present cold weather afforded an opportunity too favourable to be lost, of ascertaining by experiment, whether the water obtained from the melted ice of sea-water be free from the taste of salt or not; of comparing its gravity with that of the sea-water, &c.; and of finding the degree of cold in which the latter begins to freeze: and I beg leave to lay before you an account of my researches in these matters, and of the methods I followed in making them. If you, SIR, should think them worthy of notice, and would communicate them to the learned body over which you preside, you would confer an honour on, &c.

THE ſea-water uſed in the following experiments was furniſhed by Mr. OWEN, who keeps the Mineral Water Warehouſe, at Temple Bar; who aſſured me, that it was taken up off the North Foreland.

On the 27th of January, 1766, at ten o'clock in the evening, I filled a jar $3\frac{1}{4}$ inches in diameter and $6\frac{1}{2}$ inches deep, with ſea-water, and expoſed it to the open air, the thermometer ſtanding at 15°. At noon the next day, on taking it in, I found it frozen very hard, except a very little at the bottom, which remained quite fluid: I now ſet it by a ſtove in a heat of 56° to thaw. The ice when taken in from the open air was one quarter of an inch above the edge of the jar. When the jar had continued in the degree of heat abovementioned during eight hours, I took out the ice, which was then $3\frac{1}{2}$ inches long and two inches in diameter; about two-thirds of the water appeared to remain. In order to clear the ice from any brine that might adhere to it, I waſhed it in a pail of pump water, in which it was ſuffered to remain about a quarter of an hour, and then ſet it in a ſieve to drain off the water in which it had been waſhed.

On the 29th of January, 1776, I ſet the beforementioned ice in a baſin in a heat of about 46°, in which it continued nine hours before the whole was diſſolved. The bulb of a thermometer reſted on the ice during the time of the ſolution, and continued without variation at 32°. The water thus obtained was, to my palate, perfectly free from any taſte of ſalt.

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In order to aſcertain the comparative gravity of this water, I filled a bottle with it to a certain mark in its neck, which was very narrow, and weighed the bottle ſo filled very carefully. I weighed the ſame bottle, filled to the ſame mark in its neck with ſea-water and other waters ſucceſſively, which were all brought to the ſame degree of heat by a thermometer. The reſults were as follow; *videlicet*,

	Grains.
Water obtained from the melted ice of the ſea water,	1614
Diſtilled rain water,	1612
Water taken out of a water tub, being a mixture of rain and ſnow water,	1615
The ſea water,	1653
The *reſiduum* of the ſea water from which the ice before mentioned had been taken,	1659

To find the degree of cold in which ſea water begins to freeze, I made the following experiments.

I expoſed to the open air a decanter filled with the ſea water, in which a thermometer was ſuſpended, the bulb of which reached to the middle of the wideſt part of the decanter; a jelly glaſs filled with the ſame ſea-water, in which alſo a thermometer was put, reſting on the bottom, was placed in the ſame expoſure. The reſult will be ſeen in the following table:

 Veſſel.

January 29, 1776.

Veſſel.	Time.	Immerſed Therm.	Therm. in the open Air.	Effects, &c.
	h ′			
Decanter,	11 30 A.M.		19	
Jelly glaſs,				
Decanter,	12 0	33	19	A number of beautiful feathered cryſtals appeared in the jelly glaſs; they began to ſhoot from the top, which was covered with ice, toward the bottom; when they reached it, the thermometer roſe immediately from 25 to 28.5.
Jelly glaſs,		25 to 28.5		
Decanter,	12 15	31	19	Ice began to form in the decanter, though hardly perceptible at the edge of the water.
Jelly glaſs,		28.5		
Decanter,	12 20	30		
Jelly glaſs,		28.5		
Decanter,	12 30	29	19	Cryſtals of a laminated appearance began to ſhoot downwards obliquely from the ice at the ſurface, which at the edge of the water was barely two-tenths of an inch thick; no appearance of ice in the middle of the ſurface.
Jelly glaſs,		28.5		
Decanter,	1 0 P.M.	27.5	19	Cryſtals began to ſhoot round the neck of the decanter cloſe to the glaſs.
Decanter,	1 15	28.5	19	The inſide became covered with finely feathered cryſtals, which made it impoſſible to obſerve the height of the thermometer, without raiſing it till the quickſilver in the tube appeared above the ice.
Decanter,	4 0	28.5	19	

January

January 29, at eight o'clock in the evening, I expofed to the open air two fimilar jars, each $5\frac{1}{2}$ inches deep and $1\frac{7}{10}$ inch in diameter; one of which I fhall, for the fake of diftinction, call A; the other, B. A was filled with the fea water; B with water taken out of a water tub, which was a mixture of rain and fnow water. In A two thermometers were placed; one refted on the bottom; the upper part of the ball of the other was a quarter of an inch only below the furface of the water; one thermometer was alfo placed in B, refting on the bottom. The following table fhews the refult.

Veffel.	Time.	Therm. at the Top.	Therm. at the Bottom.	Therm. in the open Air.	
	h ′				
A		60	60	19.5	
B	8 0 P.M.		60		
A		40	33		
B	8 15		38		
A		35	29.5		
B	8 20		37.5		The furface of the water in B covered with ice.
A		31	26.5		
B	8 25		34		Surface as before.
A		29	25		No appearance of ice.
B	8 30		32		The ice on the furface increafed.
A		28.5	24.5		Ice began to appear on the furface.
B	8 32		32		Quite frozen.
A		28.2	28.5		Cryftals over every part of the glafs.
B	8 36		32	20	As before.

N. B. During the time in which thefe obfervations were made, the thermometer in the open air rofe half of a divifion.

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The following table ſhews the reſult of ſome further obſervations on the effects of cold on the ſea-water in the jar A of the laſt table, which had been thawed in order to be now expoſed again to the open air. The thermometers in the jar continued in the ſame ſituation as before.

January 30, 1776, A. M.

Time.	Therm. at the Top.	Therm. at the Bottom.	Therm. in the open Air.	Effects, &c.
h ′				
10 32	34.5	35.5	16.5	The water fluid.
10 39	29	32		Ice began to be formed about the glaſs at the edge of the water.
10 42	28.5	30.5		Still continued to have ice only about the edge of the water.
10 48	28	28		The ſurface of the water rendered ſtagnant by the ice.
11 1	27	24.5	18.5	The cryſtals had almoſt reached the bottom.
11 $1\frac{1}{2}$ } 2	27 +	28.5		During the half minute employed in this obſervation, the cryſtals reached the bottom of the jar; the lower thermometer roſe almoſt inſtantaneouſly from 24.5 to 28.5, and was immediately rendered obſcure by the ice.
11 45	26.5	28.5	19	The jar was taken in from the open air, and the lower thermometer lifted out of the ice to a ſufficient height for the obſervation.

From theſe obſervations it ſeems that the freezing point of ſea-water ſhould be fixed in FAHRENHEIT's ſcale at 28.5.

As the water, when it began to freeze in two experiments, exhibited phenomena different from any I had obſerved before, it may not be improper to ſubjoin an account of them.

At

At fourteen minutes after eight in the morning of January 31, I put the jar B of the ſecond table, containing the ſame water; *viz.* a mixture of rain and ſnow water, in a window, having the evening before placed a ſecond thermometer in it, the bulb of which was juſt below the ſurface of the water. This as well as the thermometer at the bottom ſtood at 27.5, and the water was perfectly fluid; the thermometer placed near the jar within the window was at 23.5. At twenty-ſeven minutes after eight it began to freeze at the bottom of the jar, the thermometers at the top and bottom ſtanding alike at 27. The inſtant the cryſtals began to encompaſs the ball of the thermometer below, which they very ſoon did after it began to freeze, the quickſilver roſe in it to 32°, the upper one continuing at 27°. The cryſtals continued to ſhoot upward, and in leſs than half a minute reached the bulb of the thermometer at the ſurface, which immediately roſe to 32°.

At ten minutes before ſix in the evening of the ſame day, I put the jar A of the ſecond table into the open air, its contents the ſame; *viz.* ſea water. The thermometers in it were likewiſe the ſame, not having been moved; they both ſtood at 34°; that in the open air at 19.5. At ſix o'clock the thermometer above was at 31°, that below at 28.25. At this time I diſcovered ſome ice on the ſurface of the water; but as it was by candle-light, I could not diſcern its firſt appearance. At ten minutes after ſix, the thermometer above was at 29°; that below at 26.5. At fifteen minutes after ſix, the upper thermometer at

28.5; that below at 25°. At ſeventeen minutes after ſix, both the thermometers ſtood at 28.5, cryſtals having riſen from the bottom covered the ball of that below, on which it roſe inſtantly from 25° to 28.5. The thermometer in the open air continued as at firſt; *viz.* at 19.5.

The ſcale of all the thermometers uſed in theſe experiments was FAHRENHEIT'S. I have ſent herewith ſpecimens of the water; *viz.* of the ſea-water; of the water procured from its melted ice; and of the *reſiduum* of the ſea-water from which the ice was taken.

XV. *Eaſy*

XV. *Easy Methods of measuring the Diminution of Bulk, taking place upon the Mixture of common Air and nitrous Air; together with Experiments on* Platina. *By* John Ingenhousz, *M. D. F. R. S. Physician to their Imperial Majesties at* Vienna. *In a Letter to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Vienna, Nov. 3, 1775.

SIR,

R. Feb. 15, 1776.

SOME time ago I amused myself with some experiments relating to nitrous air. Having received from the learned Abbé FONTANA a copy of a pamphlet, which he published this year under the title, *Descrizione e usi di alcuni stromenti per misurare la salubrita del aria, di Felice* FONTANA. *In Firenze, l'anno* MDCCLXXV: *per Gaetano Cambiagi Stampatore granducale*, which most probably will already be known to you; I imitated some of them, and found them very useful for the intended purpose of measuring the quantity of air absorbed or diminished by mixing the nitrous with the common air; by which *criterion* the degree of the salubrity of common air may be ascertained according to the discovery of Dr. PRIESTLEY. Abbé FONTANA first produces nitrous air in a separate vessel, and then forces it into the glass, or other vessel, in which it is to remain, till a communication be opened between this vessel and the other

other which contains common air. I found it a difficult matter to force always juſt the ſame quantity of nitrous air into the veſſel; becauſe I could never be ſure that the nitrous air had diſlodged all the common air out of it, or had diſlodged always the ſame quantity of common air. If this quantity is not always juſt the ſame, ſome variety muſt happen in every experiment; and thus an exact valuation of the quantity of air abſorbed cannot well be made. To obviate in ſome meaſure this difficulty, and to abridge the experiment by mixing ſuddenly the two airs together, I contrived the inſtrument of which I ſend you here a drawing. It is a ſtrong glaſs veſſel, nearly two inches and a half in diameter, and about as much in height: a conical figure would perhaps be better. A braſs cover, which embraces the glaſs about half an inch downwards, is cemented to it, and has a hole in its middle, correſponding with the hole in the glaſs veſſel. This hole of the braſs cover has a female ſcrew fitted to receive the male ſcrew of a braſs tube, about ſeven inches long and about an inch in diameter, terminating at one end in a male ſcrew (adapted to the abovementioned copper plate) and at the other, in a neck adapted to enter the mouth of an elaſtic gum bottle, otherwiſe called *boradchio* or *caout-chouc*, to be tied to it with a ſtrong ribbon. This braſs tube has towards each extremity an air-tight cock, by which the communication between one extremity and the other may be opened or ſhut. Between theſe two cocks, about the middle of the tube, is a ſhort lateral tube, communicating

nicating with the canal of the other tube. This lateral tube has alſo an air-tight cock, which opens or ſhuts up the communication with the long tube, and has a female ſcrew to receive the male ſcrew of another ſhort tube, which ſerves to receive a glaſs tube bent at right angles and of two feet or more in length; the diameter ſomewhat more than that of a large quill. This glaſs tube is to be divided into any number of equal parts. I uſe the inſtrument in the following manner. The elaſtic gum-bottle being well tied to the braſs tube, all the cocks ſhut, and the glaſs tube fixed to its place, I pour a certain quantity of *aqua fortis* (v. g. ℥ ſs) into the glaſs veſſel, taking care that none of it touches the braſs cover: then I put into it a certain quantity of iron filings (v. g. ʒj) wrapt up in a bit of paper to prevent its being immediately corroded. This being done, I ſcrew the glaſs veſſel to the braſs tube, ſo that no air can get out. When the red fumes begin to riſe, I open the two cocks of the braſs tube, which open the communication between the glaſs veſſel and the elaſtic gum-bottle. By ſqueezing the elaſtic gum-bottle, I force the two airs to mix together. The diminution of the air is ſoon perceived by the elaſtic gum-bottle becoming flaccid. When I judge the air is as much diminiſhed as it can be, I put the extremity of the glaſs tube into a veſſel with water, and open the cock of the ſide tube: the water immediately riſes in the glaſs tube to a height proportioned to the diminution of the two airs. By repeating ſeveral times the experiment in the ſame place, I found the

riſe of the water nearly the ſame, though not ſo exactly as I could have wiſhed: the variation I aſcribed partly to the elaſtic bottle not being always of the ſame firmneſs or elaſticity, which it loſes more or leſs by ſqueezing. I contrived another method more ſimple, and perhaps more accurate, which is the following: I took a glaſs tube about two and a half feet long, and not quite a twelfth of an inch in diameter; ſo that a column of quickſilver might ſlide through the whole without diſperſing itſelf, filling always the whole cavity. I cemented to each extremity a braſs ring, that I might be able to ſhut the opening with my finger without hurting myſelf. This tube being divided into 100 equal parts, I uſed it in two different ways; *viz.* having poured ſome *aqua fortis* into a little phial, and put to it ſome filings, I thruſt the extremity of the glaſs tube, into the neck of the phial. A column of quickſilver of about an inch in length occupied that end of the glaſs tube which was in the neck of the phial. The whole was kept in ſuch a poſture that the tube was nearly in an horizontal line, the end which is put into the phial being rather the higheſt. Care was taken that the tube ſhould not touch the *aqua fortis*. The phial being filled with red fumes and the extremity of the tube ſurrounded with them, I open and ſhut alternately the oppoſite extremity of the tube, ſo as to allow the quickſilver to advance ſlowly towards the middle; as ſoon as the column of quickſilver is arrived at the middle, I take the tube out of the bottle, and ſhut each extremity with the fore-finger: thus moving the tube upwards and downwards as briſkly as can be done with a certainty of keeping both extremities all

all the while exactly ſhut. The two airs being thoroughly mixed, I put one extremity into a veſſel filled with quickſilver, and withdrawing the finger from the opening, the quickſilver riſes immediately within the tube, and ſhews by its height the exact quantity of air diminiſhed. The other method is this: I tye to the end of the ſame tube the neck of a ſmall elaſtic gum-bottle, the bottom of which is cut away: having put ſome iron filings into a little phial, filled with *aqua fortis*, I put the end of the tube within the mouth of the phial, clapping my hand faſt to the orifice of the phial, the looſe part of the elaſtic bottle, ſo that the nitrous air, riſing from the phial, muſt take its courſe through the tube. When the whole tube is filled with red fumes, I take it out, and ſhut the two extremities with my two fore-fingers. Then I put one end of the tube in a veſſel with quickſilver, and withdraw both fingers for an inſtant, to make the column of quickſilver riſe within the tube. I apply immediately both fingers; and holding the tube nearly in a horizontal direction, ſo that the extremity where the quickſilver is may be rather the higheſt, I open and ſhut at the ſame time both extremities, ſo that the column of quickſilver gradually advances towards the middle. The quickſilver advancing towards the middle, as much common air follows the quickſilver as it forces out nitrous air from the other extremity. As ſoon as the column of quickſilver is in the middle, I keep both extremities well ſhut with my fingers, and moving the tube in various ways, I force the two airs to come into mutual contact, and to mix intimately together. Then I put one

extremity into a veſſel filled with quickſilver, withdraw the finger from within the quickſilver, and obſerve to what height the quickſilver riſes. It requires ſome practice to perform this experiment with dexterity.

Some time ago I got ſome ounces of fine *platina* from Spain, through the means of his excellency Count DIETRICHSTEIN, with which I made ſome experiments. Moſt writers aſſert, that a conſiderable part of the *platina* is attracted by the magnet, but not the whole of it: but by a nice inquiry I found, that every one of the particles obeyed the magnet more or leſs, except ſome tranſparent ſtony particles; and that even theſe were all magnets in themſelves; or that each particle had two poles, which I could change at pleaſure by the application of magnetical bars. Though their magnetical virtue is always much leſs than that of particles of iron, yet every one had more or leſs of it; but ſome ſo little as not to be perceived but by applying a ſtrong magnet to them when floating upon water. Beſides the flat, ſmooth, and ſhining bright particles, which are alone the true *platina*, I find two other kind of particles among them; *viz.* ſome very ſmall black particles, moſt of which are of an irregular figure, reſembling the iron ſand found in ſome parts of North America; at Teneriffe; near ſome lakes in Italy; in ſome rivers in Tranſylvania, among the gold duſt, which is taken out of them; and in many other places. Some of theſe black particles, though few in compariſon with the number of the irregular particles, are of a very regular figure; and when ſeen through a good magnifier, ſomewhat reſemble the figure of a melon.

3 Both

These black particles of both sorts, I find, are attracted by the loadstone, and have each of them two poles, though those of an irregular figure have them more manifestly (a). The other particles are of a gold colour; having, in general, more or less of a paleness approaching to the colour of *platina*. Some of these gold particles have the figure of the rest of the *platina*, differing only from them in colour, and in not being so bright, or as it were polished. Others are irregular masses of indeterminate figure having generally a spungy appearance. The most part of these gold particles were evidently attracted by the magnet, and shewed upon the surface of the water their two distinct poles. These gold particles being put upon a piece of charcoal, and the flame of a candle directed upon them by the blowpipe of the chemical pocket laboratory, described by GUSTAVE VON ENGESTROM, published in the English translation of CROWNSTED'S *Mineralogie*, run easily into round balls, which have all the appearance and quality of real gold, except their being in general magnetical or having two distinct poles. I make no doubt but this magnetical quality is owing to some *platina* mixed with the gold. I could never melt a single particle of true shining *platina* by blowing strongly upon it with the blowpipe; the only change they underwent by this operation was to lose their brightness and the greatest part of their

(a) If magnetism is a criterion of iron, there must be iron in the *platina*; but if the rest of this substance be gold, according to some, why should not this be precipitated together with the gold added to it, by the addition of a solution of green vitriol to the *aqua regia* in which the two metals are dissolved?

mag-

magnetical virtue. Having filled a ſmall glaſs tube with that *platina*, I found each end of it attracted both poles of a compaſs indiſcriminately; but being put to a ſet of magnetical bars, it became a real magnet, having two diſtinct poles, which I could change at pleaſure. I filled another ſmall tube with *platina*, the hollow of the tube being only of ſuch a ſize, as to allow the particles of *platina* to go in freely. I ſtuck a pin in each end, and fixed the pins with ſealing wax. I directed five or ſix electrical exploſions from three very large jars through the tube; after which, I found the *platina* had acquired no polarity. By looking with a microſcope at the outſide of the tube, I found the *platina* was much changed, ſo as to appear one uninterrupted cylinder of metal, all the interſtices between each particle being quite, in appearance at leaſt, obliterated and filled with bright metal. The places which were not bright, were become of a black hue, and appeared to be parts of the *platina* not melted; which I found afterwards to be the caſe. I attempted to ſhake the particles out of the tube, but I could not ſucceed. I could only force out ſome few at the opening with a pin. I ſeparated a little bit of the tube with a file, to puſh out the cylinder of *platina*; but could not ſucceed without employing a great force: therefore I beat ſome part of the tube to pieces with a hammer, and found each particle had undergone a remarkable alteration. All of them appeared in ſeveral places to have been melted, and ſome little ones ſeemed to have been intirely in a fluid ſtate; they all adhered in lumps together ſo ſtrongly, that many of them

them could abſolutely not be rubbed aſunder between the fingers. The inſide of the tube exhibited marks of having received impreſſions of the melted metal. By comparing the ſeparated particles of this *platina* with particles not expoſed to an electrical exploſion, they were ſcarce to be known for the ſame ſubſtance. I had put ſome iron filings in a tube of the ſame ſize, and directed the ſame exploſion through it, in order to compare the effect of electricity upon it with what happened to the *platina*. I found, by looking at the outſide, ſomewhat of the ſame appearance of being melted. By cutting this tube in ſmall bits, I could eaſily puſh out the filings with a pin, which I could not do in the other caſe but with great force. The filings ſtuck together, as the particles of *platina* had done; but with leſs force. By this experiment it ſhould ſeem as if *platina* (which hitherto could never be melted by common fire by itſelf, but only in the *focus* of a very ſtrong burning glaſs, ſuch as was a little while ago made at Paris) were equally fuſible, if not more ſo than iron, by electrical fire. I was ſomewhat ſurprized to find, that the particles of *platina* taken out of the aforeſaid tube, had got a remarkably ſtronger magnetical force, being attracted by a loadſtone at a greater diſtance, and turning their poles more briſkly upon the water than before, though the whole cylinder of theſe particles, ſtill incloſed in the tube, gave no ſigns of having acquired polarity. Thus it appears, that common fire diminiſhes the magnetical virtue of *platina*, and that electrical fire increaſes it; which I thought the more pro-

 bable,

bable, becauſe thoſe very particles, which had acquired by electricity their increaſed magnetical force, did loſe it again after being heated upon a piece of charcoal, which did not happen in the particles of iron. *Platina* mixed with lead was put upon an ordinary cupel in a docimaſtic furnace ſtrongly heated. When the metal came to a ſolid ſtate, it was a flat rough lump, much heavier than the crude *platina*. I put freſh lead to it, and cupelled it again as before. I repeated it ten times, when I obtained a large lump, ſomewhat leſs flat, pretty ſmooth, but not bright; of about the ſame weight as after the firſt cupellation(b). This lump did not give the leaſt ſign of magnetiſm, and even would not receive any by being applied to ſtrong magnetical bars (I forgot to try this after the firſt cupellation) and the ſubſtance was very brittle, nearly of the ſame colour as *platina*, and took a fine poliſh. If it could tend to any uſeful purpoſe, I would repeat theſe experiments oftener, to be quite ſure whether the event would be conſtantly the ſame.

Though a piece of ſoft iron attracts the two poles of a compaſs indiſcriminately, and is incapable of acquiring polarity itſelf, yet I have never been able to ſeparate a ſingle particle of the ſofteſt iron, even when I ſeparated it carefully with a flint, or other body containing no ſteel or iron, without its giving evident ſigns of two diſtinct poles when floating upon water, nay even upon paper. I could alſo never find iron filings of ever ſo ſoft

(b) I loſt the paper that contained the exact weight before and after the cupellation.

a ſubſtance,

a ſubſtance, but each particle ſeparately had evidently two poles. Such iron filings mixed with bees wax, as much as is ſufficient to keep them together, got a ſtrong polarity by being touched with magnetical bars, and had all the qualities of a magnet: the maſs is eaſily cut with a warm knife, and is very convenient for magnetical experiments, ſuch as Dr. KNIGHT made with ſimilar loadſtones made of pounded magnets. I found alſo, that each particle of thoſe granulated iron ores of Sweden, which are placed among the *mineræ ferri retractoriæ*, ſeparated iron from ſtone, and had two diſtant poles; and that a piece of the ore itſelf became a tolerable good magnet by being touched with the bars.

I am, &c.

XVI. *An Account of Three Journeys from the* Cape Town *into the Southern Parts of* Africa; *undertaken for the Discovery of new Plants, towards the Improvement of the Royal Botanical Gardens at* Kew. *By Mr.* Francis Maſſon, *one of his Majeſty's Gardeners. Addreſſed to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR, Kew, Nov. 1775.

R. Feb. 1, 1776.

IN compliance with your requeſt, I now ſend you the account of my firſt journey from the Cape, which I have tranſcribed from my journal; and if you ſhall find it to contain any thing worthy the notice of the Royal Society, I beg you would do me the honour to preſent it to that illuſtrious Body; and believe, that with the greateſt pleaſure I ſhall communicate to you and to them the remaining part of my obſervations.

I am, &c.

AN

AN ACCOUNT OF THE FIRST JOURNEY.

ON the 10th of December, 1772, I ſet out from the Cape Town, towards the evening, attended by a Dutchman, and a Hottentot who drove my waggon, which was drawn by eight oxen; this being the manner of travelling there. They prefer oxen to horſes, becauſe they are much cheaper, and more eaſily maintained. At ſun-ſet we croſſed the Salt River, about two miles diſtance from the Cape Town, where is placed a high flag-ſtaff with a large old piece of cannon, intended to give ſignals to prevent a ſurprize from an enemy: theſe ſignals are anſwered by others, placed upon eminences at proper diſtances, and alarm the adjacent country in a ſhort time. In the night we travelled over a large ſandy plain; and towards the morning ſtopped at a ſmall cottage called Elſis Kraal. The next day we partook of the diverſion of hunting a ſmall ſpecies of antelope, which the Dutch call Steenbock. We croſſed great part of this ſandy plain, which is very extenſive, reaching from the Tyger Berg to Bay Falſo, upwards of twenty miles; from the Table Mountain to Hottentot Holland Mountains, about thirty miles. The ſoil of this plain is unfit for cultivation; being a pure white ſand, blown by the S.E. wind from the ſhore of Falſo Bay, and often forming large hillocks; it is, nevertheleſs, overgrown with an infinite variety of plants peculiar to this country.

11th, We paſſed the Tyger Berg, leaving it on our left hand; and along its ſkirts ſaw many fine plantations, abounding with corn fields and vineyards.

12th, We paſſed the Paerden Berg (that is, Horſes Mountain) ſo called from the number of Zebras formerly found there, which are called by the Dutch inhabitants wild horſes. Towards the evening, croſſing the Berg Rivier (that is, Mountain River) we entered into the diſtrict called Draakenſteen, a valley about ten miles in length, and about five in breadth; containing many large plantations of vineyards, and orchards of moſt kinds of European fruit, which have been tranſported hither by the Dutch; *viz.* apricots, peaches, plumbs, apples, pears, figs, mulberries, almonds, cheſnuts, and walnuts; but no Indian fruits, except the guyava and jambo, neither of which ripen well. Theſe plantations are generally ſituated near the foot of the mountains, and watered by ſmall ſtreams, which deſcend with great rapidity, and are conveyed all over their gardens and vineyards.

16th, We travelled to a ſmall village called Perel, ſo named from its ſituation on the N.E. ſide of a hill called Perel Berg. In it is a church and about a dozen of houſes diſperſed along the foot of the hill, with pretty gardens and vineyards, which produce excellent wine.

17th, I went up to the top of the Perel Berg, where I ſpent a whole day in ſearch of plants, and hunting a ſort of antelope called Ree Bock; but had no ſucceſs. I ſaw nothing here ſo worthy of obſervation as two large ſolid rocks, of a roundiſh figure; each of which, I may poſitively

tively ſay, is more than a mile about at the baſe, and upwards of two hundred feet high above the ground. Their ſurfaces are nearly ſmooth, without chink or fiſſures, and they are found to be a ſpecies of *ſaxum* or granite, different from that which compoſe the neighbouring mountains.

18th, From hence we continued our journey to a valley, adjoining the S. E. part of Draakenſteen, called Franſche Hoek[(a)]; it having been ſettled by a party of French refugees, who left France about the beginning of this century. Though but a poor ſettlement, being a cold, mooriſh ſoil, it produces corn enough for its inhabitants, ſour wine and ſome fruit. Drakenſteen and Franſche Hoek are bounded on the N.E. and S.E. by a chain of high mountains, which have their beginning at Cape Falſo, run in a winding courſe to the N.W. of St. Helena Bay, and ſend out ſeveral branches into the interior parts of the country. Theſe two vallies are watered by the Berg Rivier, which riſes in the Stellenboſch mountains. It is a conſiderable river, but no where navigable. The banks are decorated with a great variety of uncommon trees.

January 4, 1773, We reached Stellenboſch, a ſmall village about thirty miles N.E. from the Cape Town, conſiſting of about thirty houſes, forming one regular ſtreet, with a row of large oak-trees on each ſide along the front of the houſes, which render it very pleaſant in

(a) This, I ſuppoſe, to be the place which ſome of the French voyagers in their obſervations on the Cape of Good Hope, call Petite Rochelle.

the

the hot ſeaſon. Theſe oaks, which are of the ſame ſort with ours in England, were brought out of Europe by ADRIAN VANDERSTELL, formerly governor of the Cape, who built this village, and gave it his name. The country round it is populous, and contains many rich farms, which produce plenty of corn and wine. It is watered by a ſmall river called Eerſte Rivier, which diſcharges itſelf into the Eaſt part of Falſo Bay. The farmers we found buſy in treading out their corn; which is performed by horſes in the following manner. They make a circular floor about thirty, forty, or fifty feet diameter, with a compoſition of clay and cow-dung, which binds very hard; round it they erect a mud wall, about breaſt high; this floor they cover with ſheaves, beginning in the middle, and laying them in concentric circles till they reach the outſide. They then turn in about twenty or thirty horſes, which a Hottentot, furniſhed with a long whip, drives round and round till the corn be trodden out, and the ſtraw become as fine as chaff; which they afterwards clean, and carry into their granaries. This method they can practice with great ſecurity, as it ſeldom rains here from the middle of October to the middle of March.

5th, From thence we travelled along the foot of the Stellenboſch mountains to Hottentot Holland, a pleaſant a[illegible] country; ſurrounded on three ſides by the mountains, and the other opening to the Eaſt part of Falzo Bay; in it are eight or ten plantations, with elegant

gant houſes, gardens, vineyards, and corn fields: this country lies about thirty-five miles Eaſt from the Cape Town.

6th, We aſcended the mountains by an exceedingly ſteep rugged path, which the peaſants call Hottentot Holland Kloof[b], and after much labour and fatigue gained their ſummit, when we entered a ſpacious plain, interſperſed with an infinite number of large fragments of rocks, viſibly decayed by the force of the S.E. wind, which blows here during the ſummer with very great force. Some of theſe rocks appeared like the ruins of church-ſteeples, and were worn ſo thin with wind and rain, that the ſofter parts of them were perforated in many places. They are formed of the *cos quadrum* of LINNÆUS. The ſoil about them is a black earth intermixed with a pure white ſand, probably proceeding from the decay of the rocks. Theſe mountains abound with a great number of curious plants, and are, I believe, the richeſt mountains in Africa for a botaniſt. We then paſſed the Palmet Rivier, ſo called by the peaſants from a plant[c] which almoſt covers the water; the leaves of which greatly reſemble that of the ananas or pine-apple, but their flowers are like thoſe of a reed. At night we croſſed a ſmall river, called Boter Rivier; and took up our lodging at a mean cottage, where the Dutchmen and Hottentots live almoſt promiſcuouſly together, their beds conſiſting only of ſheep's ſkins. The next morning an

(*b*) Kloof, is a narrow paſſage over the lower part of a chain of mountains, or ſometimes a narrow paſſage between mountains.

(*c*) *Schoenus ſerratus.*

 old

old Hottentot brought out a fat wether, and ſlaughtered it; part of which we ate for our breakfaſt.

8th, We came to a hot bath, ſituated on the S.E. ſide of a large mountain called Zwart Berg (d). The India Company have erected here a tolerable houſe for the reception of ſick people. The water is ſcalding hot where it ſprings out of the earth; but after being conveyed about ten or twelve paces to the bath, it becomes more temperate. The people here ſeem to uſe it for all diſeaſes without exception, and often perhaps receive more hurt than benefit by it.

10th, We croſſed Rivier Zonder Eynde; that is, Endleſs River, which diſcharges itſelf into the Breed Rivier (e). At night we came to Sweet Milk Valley, where there is a good houſe belonging to the overſeer of the Company's woods; who received us with great civility, and kept us with him five days. The fourth day, we went into the woods, which are about half-way up a high chain of mountains that extends along the N. and N.E. ſide of the the valley. I was accompanied by a farmer's ſon, who took with him eight large rough dogs, which in our way ſtarted two wolves; one of them we wounded with ſmall ſhot, ſo that the dogs overtook him. A fierce battle enſued, which laſted an hour before he was killed. We afterwards climbed over many dreadful precipices till we arrived at the woods; which are dark and gloomy, interſperſed with climbing ſhrubs of various kinds. The trees are very high; ſome from eighty to a hundred feet;

(d) Black Mountain. (e) Broad River.

often

often growing out of perpendicular rocks where no earth is to be feen. Among thefe the water fometimes falls in cafcades over rocks two hundred feet perpendicular, with awful noife. I endured this day much fatigue in thefe fequeftered and unfrequented woods, with a mixture of horror and admiration. The greateft part of the trees that compofe them are unknown to botanifts. Some I found in flower; others, which were not fo, I was obliged to leave for the refearches of thofe who may come after me in a more fortunate feafon.

16th, I vifited a Hottentot Kraal: the men were all, at this time, attending their herds; but the women and children were employed in building their huts; which are very low, of a circular figure, and made of flender poles, the ends of which are ftuck into the ground, fo as to form a number of arches croffing one another; thefe they afterwards cover with mats made of reeds. They have a round hole in the middle of the floor, in which they make the fire, and fit all round it upon the ground; but have no chimney or hole to let out the fmoke.

18th, We croffed the Breed Rivier, which is confiderable, and only paffable in winter by a ferry; but at this feafon we forded it at the place where the Rivier Zonder Eynde joins it. At night we arrived at Schwellendam, a place about 150 miles N.E. from the Cape Town, where we remained two days; but finding the feafon too far fpent for making any confiderable collections, I returned back to the Cape by the fame road I came. It was on this

journey that I collected the ſeed of the many beautiful ſpecies of *ericæ* which, I find, have ſucceeded ſo well in the Royal Garden at Kew.

SECOND JOURNEY.

R. Feb. 22, 1776. MY ſecond journey was performed in company with Dr. THUNBERG, a native of Sweden; who was ſent out by the Dutch to collect plants at the Cape, and is on that errand now in the Dutch Eaſt Indies.

Sept. 11, 1773, We left the Cape Town, directing our courſe along the N.W. Coaſt. We paſſed the Blue Mountains; but the weather proving rainy, and attended with a fog, we loſt our intended road, and were obliged to lodge that night in the fields.

12th, We came to Groene Kloof, a place belonging to the Eaſt-India-Company, where we remained ſeveral days, the weather being rainy and unſettled; during which time we made ſeveral excurſions along the ſides of the hills, and alſo over a large ſandy deſart towards the ſea ſhore; where we found a great variety of beautiful plants, and ſeveral animals peculiar to this climate; *viz.* antelopes, oſtriches, and plovers of ſeveral ſorts.

19th, From Groene Kloof we paſſed a ſmall hill, called Konter Berg; and from thence entered a large barren country, named Zwart Land (a). The earth is a grey ſand,

(a) Black Land.

level for many miles, and covered over with low ſhrubs of various ſorts. At night we came to a farmer's houſe, where we remained two days, ranging the adjacent fields, in which we found many curious plants, and ſhot ſeveral animals, as ſteenbocken, hares, partridges.

22d, Still, in the ſame direction, we travelled over a deep, ſandy country with great fatigue; when, towards the evening, we arrived at Saldana Bay. Here we lodged with a farmer on the Eaſt ſide of the bay; but being deſirous of croſſing to a houſe inhabited by ſome of the company's ſervants, who during the ſummer ſeaſon ſhoot ſeals for oil, on the 23d we made a ſignal for them to ſend their boat; which they immediately anſwered, and brought us over the bay, which is about two miles in breadth, where we were hoſpitably entertained by the maſter. I obſerved, that the direction of this bay is laid down wrong in all the maps that I have ſeen, except that of the Abbé DE LA CAILLE; they have given it a right Eaſt direction, whereas it has nearly a South direction, almoſt parallel to the ſea coaſt, and, I ſuppoſe, almoſt twenty miles in length. The entrance of the bay is difficult, having ſeveral ſmall iſlands in it, and the adjacent country being little better than a ſandy deſart, and the water brackiſh; it is, I think, improper for ſhipping. It lies about fifty miles N.N.W. from the Cape Town. We found here great variety of curious plants; and in particular, a large bulbous root, growing on dry precipices, which the Dutch call *vergift-boll*, poiſon bulb; the juice of which, they ſay, the Hottentots uſe as an ingredient to poiſon their arrows.

We

We found it to be a ſpecies of *amaryllis*, and, by the leaves growing in a fan ſhape, we called it *amaryllis diſticha*.

27th, From Saldana Bay we journeyed to Witte Klip (White Cliff) being a white granite ſtone of an enormous ſize; from the top of which we had a charming view of the ſea coaſt from St. Helena Bay to the Cape of Good Hope. The whole country affords a fine field for botany, being enamelled with the greateſt number of flowers I ever ſaw, of exquiſite beauty and fragrance. Here we ſaw numbers of wild dogs, and ſome of them ſo near that I could diſcern them to be about the ſize of a large fox-hound. They go in large packs, and do great damage to the cattle. They alſo deſtroy the antelopes wherever they go, by hunting them down in the ſame manner as our hounds do a ſtag.

30th, To St. Helena Bay, where the Berg Rivier diſcharges itſelf, which is here very deep, and bordered on each ſide by extenſive marſhes that are impaſſable, and overgrown with very high reeds. Thoſe reeds are plentifully ſtocked with birds of various ſorts, which build their neſts upon ſuch of them as hang over the water. There is one bird, in particular, which has a wonderful effect among the green reeds; its body being a bright crimſon, with black and grey wings; and by the brightneſs of their colours, when ſitting among the reeds, they look like ſo many ſcarlet lillies: this is the *loxia orix* of LINNÆUS. There are ſtill ſome of the ſea horſe, or *hippopotamus amphibius*, in this river; but it is now prohibited to ſhoot any

any of them, as they are nearly deſtroyed for 800 miles from the Cape. The farmers ſhoot them for their fleſh, which they eſteem as good as pork; and of their hide, which is extremely thick, they make whips. There happened at this time a great flood, that prevented our croſſing the river at this place, and obliged us to travel four days up the river to a ferry, which greatly retarded our journey, and occaſioned many difficulties by the deepneſs of the ſand and brackiſhneſs of the water; nor is there any wine or fruit in this part of the country, owing to the ſaltneſs of the ſoil.

Oct. 6th, We came to the pont or ferry, where we collected a great number of beautiful plants, particularly *ixiæ*, *irides*, and *gladioli*.

7th, We croſſed the Berg Rivier, and entered a fine plain country, called 24 Rivieren Diſtrict; ſo called from the number of ſmall rivulets which run through that diſtrict, and diſcharge into the Berg Rivier. Here we had ſome ſour wines, and oranges and lemons in great plenty.

9th, We paſſed a branch of that chain of mountains which I mentioned in my firſt journey. They continue for many miles further to the N.W. gradually diminiſhing in height to the Weſtern ſhore. This paſſage over the mountains is called Kartouw, and is remarked for being one of the moſt difficult in this part of Africa; which we found true, being obliged to lead our horſes for three hours amidſt inceſſant rain, which made the road ſo ſlippery that, by often tumbling among the looſe ſtones, they had their legs almoſt ſtripped of the ſkin;

ſkin; and the precipices were ſo ſteep, that we were often afraid to turn our eyes to either ſide. Towards ſun-ſet, with great labour and anxiety, we got ſafe to the other ſide, where we found a miſerable cottage belonging to a Dutchman. Being however cold and wet, we were glad to take refuge under his roof. The hut had only one room; but our hoſt gave us a corner to ſleep in, which was detached by a hanging of reed mats, where he and his wife alſo ſlept; and in the other end lay a number of Hottentots promiſcuouſly together.

10th, We croſſed the Olyfant's Rivier, nearly 130 miles North of the Cape Town, where we entered into a pleaſant valley, bounded on each ſide by very high mountains; thoſe on the Eaſt had their ſummits covered with ſnow, it being then their ſpring. This country produces good corn and European fruit in great plenty, eſpecially oranges and lemons in the greateſt profuſion; and the trees grow to a great ſize. They have alſo wine, but it is ſour and unwholeſome; which, I think, may be owing to their planting their vines in wet, marſhy places. The fruit yields watery juices, which ſeldom ripen, but produce good brandy. There is a hot bath here, which we viſited, iſſuing from the ſide of a mountain. The water was nearly boiling hot at the place it iſſued out of the rock; and the people who uſed it affirmed that it was hot enough to boil a piece of meat. I obſerved an orange tree, which had been either raiſed from a ſingle ſeed, or planted when very young, in a ſeam of the rock where the water boiled out, which, to

my ſurprize flouriſhed amazingly, and all the ſides of the baſon where the people bathed were matted round with the fibres.

11th, 12th, 13th, 14th, We travelled along the banks of this river, making ſhort ſtages. The meadows yielded excellent paſture for our cattle, the graſs reaching up to their bellies, but of a coarſe texture, being chiefly *Juncus*, *ſcirpus*, and *cyperus*.

15th, We attempted to croſs the high ridge of mountains on the North ſide; but found it impracticable, having overturned our waggons on the ſide of a precipice, and greatly damaged them, which obliged us to return to a peaſant's houſe to get them repaired. This done, we held a conſultation what courſe to take; and after ſome warm debates, concluded to ſend our waggons round to a place called Rood Land, there to wait for us, while the Doctor and I directed our courſe through a country called Koud Bocke Veld, or Cold Country of Antelopes; ſo named from a ſpecies of antelopes which inhabits here, called Spring bock. This animal when hunted, inſtead of running, avails itſelf of ſurprizing ſprings or leaps, which I ſhall have occaſion more particularly to mention hereafter.

17th, We directed our courſe Eaſtward through Elans Kloof, a narrow winding paſſage through a high chain of mountains, which lies to the N.E. of Olyfant's Rivier. This road is rugged beyond deſcription, conſiſting of broken and ſhattered rocks and rugged precipices, encompaſſed on each ſide with horrid impaſſable mountains;

tains; the ſides of which are covered with fragments of rocks that have tumbled down from the ſummits at different times. We ſaw few plants here, only ſome trees of the *protea grandiflora* thinly diſperſed along the ſkirts of theſe mountains. We croſſed, in this paſſage, ſeveral ſmall rivers of the pureſt water I ever beheld, which afforded us no ſmall relief during the heat of the day. Towards the evening we entered the Koud Bocke Veld; and afterwards came to a peaſant's houſe, where we remained that night.

18th, 19th, 20th, We travelled through the Koud Bocke Veld, where we found but few plants: the face of the country being exceedingly barren, and not ſo much as a ſhrub to be ſeen. The ſeaſon here appeared to be two months later than in the neighbourhood of the Cape Town, although the diſtance be not above a hundred miles, in a direct line in a Northern direction. This country is but ſmall, containing about nine or ten Dutch places, the inhabitants of which ſubſiſt intirely by their cattle. Their winters are often ſo ſevere, that the ground is covered with ſnow for ten days together; and their early calves and lambs are often killed by the inclemency of the weather. Neither orange trees nor vines will live here, owing to the bleakneſs of its ſituation; and the boors informed us, the ſummers are often ſo unkindly, that their wheat is blighted while in ear, ſo that they purchaſe corn with their cattle from the low country farmers. The country is encompaſſed on all ſides with very high mountains, almoſt perpendicular, conſiſting of bare rocks, without

out the leaſt appearance of vegetation; and upon the whole, has a moſt melancholy effect on the mind. We ſaw ſome herds of the ſpring-bocks, a ſpecies of antelope, as obſerved before, which were ſo ſhy, that we could not come within muſket-ſhot of them.

21ſt, We deſcended by a very ſteep path into another ſmall country, called Warm Bocke Veld, encompaſſed alſo on all ſides with horrid mountains, but not nearly ſo barren. Here we had ſome ſour wine and fruit; we were alſo delighted to ſee the luxuriance of the meadows, the graſs reaching to our horſes bellies, enriched with great variety of *ixiæ*, *gladioli* and *irides*, moſt of which were in flower at the Cape in the month of Auguſt.

22d, We had a high chain of mountains to paſs before we arrived at Rood Land, where we expected to meet our waggons. Upon inquiring about the road thither of the women, with whom we had lodged the preceding night, the men being all from home, ſo that we could not procure a guide; they informed us, there was only one paſs, called Moſtart's Hoek, which was very dangerous; and that, without a guide, we ſhould run the riſk of loſing our lives, having a rapid river ſeveral times to croſs, the fords of which, by the late rains, had been rendered more dangerous than uſual. We were a little intimidated by this information; but fortifying ourſelves with reſolution we proceeded, and in an hour arrived at the firſt precipice, where we looked down with horror on the river, which formed ſeveral cataracts inconceivably wild and romantic.

romantic. This paſs, which took us near three hours march, is at the broadeſt about a quarter of a mile, but in general not above an eighth part of one. The mountains on each ſide riſing almoſt perpendicular to a ſtupendous height, had their ſummits then covered with ſnow, part of which remains till March. This river, which is the beginning of the Broad River, we had four times to croſs. The ford was exceedingly rough, the bed of the river being filled with huge ſtones, which tumble down from the ſides of the mountain; but we thought our labour and difficulties largely repaid by the number of rare plants we found here. The bank of the river is covered with great variety of evergreen trees; viz. *brabejum ſtellatifolium*, *kiggelaria Africana*, *myrtus anguſtifolia*, and the precipices are ornamented with *ericæ* and many other mountain plants never deſcribed before. At night we arrived at Rood Land, where we found our ſervants and waggons, and being a little fatigued we devoted the next day to reſt and the examination of our plants. It is to be obſerved, that during the preceding five days we had rather ſhortened our diſtance from the Cape, by reaſon of the impoſſibility of taking the waggons over the mountains with us; ſo that we were now one day's journey nearer the Cape than we had been on Bocke Veld.

26th, We travelled up a high mountain, called Winter Hoek, on the N.W. of Rood Land, one of the higheſt mountains in this part of Africa; whoſe top is covered with ſnow the greateſt part of the year. Here we expected to find plants that might endure the ſeverity of

our

our climate; but when we arrived at its top, we found nothing but a few graſſes, *reſtiones*, *elegiæ*; the whole mountain conſiſting of rock, lying in horizontal *ſtrata*, without any ſort of earth, except a little decayed rock in which the graſſes grew. From the foot of this mountain to its ſummit is a good day's journey, it being very rugged and difficult to mount. We found many curious plants growing along the borders of the ſtreams, which run in great plenty down the mountain's ſide. Rood Land is a fine level country, ſurrounded on all ſides by lofty mountains, except on the Eaſt, where the valley continues for ſeveral days journey incloſed by mountains on each ſide. Thoſe on the Northern ſide continue for ſeveral hundred miles in an oblique direction, and terminate on the Eaſtern coaſt. This country produces corn and wine in abundance, and moſt of our European fruits, which have been planted there by the new inhabitants, who are deſcendants of the French refugees; a civil, hoſpitable, and induſtrious people.

28th, 29th, We continued our journey along the banks of the Broad River, where we collected many remarkably fine flowers, particularly one of the liliaceous kind, with a long ſpike of pendulous flowers, of a greeniſh azure colour, which among the long graſs had an admirable effect (this is *ixia viridis*).

30th, We croſſed the Hexen Rivier (Witches River), which has a paſſage through the mountains, and joins the

the Broad River; this place is alſo remarkable for a hot bath.

31ſt, We paſſed on to Ko Aree Rivier, where we found many new plants; in particular, *gerania* and *ſtapelia*.

Nov. 2d, To Koekman's Rivier, the banks of which are covered with thick woods, and furniſhed with a variety of birds, which afforded us good ſport. The trees were moſtly of the *mimoſa nilotica* of LINNÆUS; the ſpecies of the birds I have not yet determined, not being provided with books upon Ornithology to ſettle one half of thoſe which I collected on this journey.

5th, We arrived at Swellendam, deſcribed in my firſt journey; and the ſame day dined with the Land Droſt, who is a juſtice of peace, and collects different taxes from the peaſants. After dinner we purſued our route to Buffel Tagt's Rivier, where is a place belonging to the Eaſt India Company. There they keep a few wood-cutters, and from thence ſupply the wheelers at the Cape, conveying their wood in waggons drawn by oxen: this place, I think, is about 150 miles from the Cape. Here we reſted five days for the benefit of our oxen, which had become very lean, and the Doctor got a freſh ſet out of the Eaſt India Company's herd.

10th, To Davenhoek's Rivier, where we remained all night, and the next morning proceeded on our journey. The Doctor imprudently took the ford without the leaſt inquiry; when on a ſudden, he and his horſe plunged over head and ears into a pit, that had been made by the *hippopotamus amphibius*, which formerly inhabited

habited those rivers. The pit was very deep, and steep on all sides, which made my companion's fate uncertain for a few minutes; but, after several strong exertions, the horse gained the opposite side with his rider.

12th, To Caffer Kuyl's Rivier. Upon our left hand, a few miles distant, we had the chain of mountains before mentioned, which here take a N.E. direction. Their summits terminate in a number of lofty, rugged pieces, which have an admirable effect. Between this chain of mountains and the sea on the S.E. lies an extensive country, to appearance low; but when one travels across it, it presents a continued series of hills and dales. The hills are quite smooth and easy of ascent, and covered with long coarse grass, which cattle seldom eat. On the declivities of these low hills grows the *aloe Socotorina* in large clumps, which when old have stems about five or six feet high, with only a few thick leaves on their tops, that at a distance appear like bands of Hottentots. The peasants make great quantities of the gum aloes from the sap of the leaves, which they sell at the Cape from two to six pence *per* pound. There is a fine species of antelope, which inhabits only here, called by the peasants Bonte Bock; something larger than a fallow deer, very shy, but not very swift.

15th, To Goud's Rivier; which at that time was about 100 yards broad, and the water came up to the seat of our saddles. On each side of this river lies an extraordinary track of land, which in the Hottentot language is called Carro. It is a dry, burning soil, of a reddish colour, intermixed

intermixed with rotten rock, and intirely divefted of grafs; but enriched with an infinite number of evergreen fhrubs, both frutefcent and fucculent: among the latter we found many new fpecies of *craffula*, *cotyledon*, *euphorbia*, *portulaca*, *mefembryanthemum*. We refolved to vifit the fea fhore, and particularly Moffel-Baay; when, late in the evening, we came to the houfe of an European, who received us very hofpitably. He was a native of Swedifh Pomerania, about feventy years old; had been fhipwrecked on the coaft of England fifty years ago, and fpoke much of the hofpitality of the Englifh. He was a man of learning, and expreffed many fenfible reflexions on the tyranny of his native country, which had forced him to feek for an afylum in the defarts of Africa. His houfe was very mean, built of mud, and miferably furnifhed; not having a bed to lie on, though he had feveral hundred oxen and fome thoufands of fheep. He had a number of Hottentot vaffals, whofe huts were fituated round his folds, where they kept feveral large fires all night long, to frighten away the wolves and tigers.

26th, We came to Moffel-Baay, which is very large, open, and expofed to the S.E. and E. The fhore is covered with fhrubs of various kinds; the greateft part of which were unknown to us, and many we did not find in flower. To the N.E. of Moffel-Baay lies a woody country, called Houtniquas Land; whofe woods, intercepted by rivers and precipices, are fo large, that their extent is not perfectly known. Thefe woods are a great treafure to the Dutch, and will be very ferviceable to the

inhabitants of the Cape, when their other woods are exhausted. In them are numbers of wild buffaloes that are very fierce, and ſome elephants; which renders travelling dangerous. We now directed our courſe Northward to the foot of the great chain of mountains, which we had again to croſs; it is there very broad, being a hard day's march from one ſide to the other. This paſs is called by the peaſants Hartiquas Kloof.

19th, We were ſeveral hours in aſcending, and after deſcending on the other ſide, we entered a valley, ſurrounded by lofty mountains: here we reſted that night by a ſtream of water, where we collected many curious plants.

20th, We continued our journey through a diſmal valley, where we ſaw neither man nor beaſt; but our labour was generouſly rewarded by the productions of the vegetable kingdom, having found ſeveral new ſpecies of plants, which for neatneſs and elegance exceeded any thing I had ever ſeen. At night we got clear of the mountains, but entered a rugged country, which the new inhabitants name Canaan's Land; though it might rather be called the Land of Sorrow; for no land could exhibit a more waſteful proſpect; the plains conſiſting of nothing but rotten rock, intermixed with a little red loam in the interſtices, which ſupported a variety of ſcrubby buſhes, in their nature evergreen, but, by the ſcorching heat of the Sun, ſtripped almoſt of all their leaves. Yet notwithſtanding the diſagreeable aſpect of

of this tract, we enriched our collection by a variety of succulent plants, which we had never seen before, and which appeared to us like a new creation.

21st, To Great Thorn River, where we encamped under a large *mimosa* tree. During the night, we had several loud claps of thunder with rain.

22d, We entered Lange Kloof, which is a narrow valley, not exceeding two miles at the broadest, and in length about 100; bounded on the S.W. by the chain of mountains beforementioned, and on the North and East by a lower ridge, which runs nearly parallel. It contains about seven or eight places, which are from twelve to twenty miles distant from each other; the houses are very mean, without walls, consisting only of poles stuck in the ground, meeting at the top, and thatched over with reeds. The people, however, are wealthy, possessing large herds and flocks. The Hottentots are in general servants to the Dutch farmers; who give them for wages beads, and tobacco mixed with hemp; the latter, which intoxicates them, they are extremely fond of. A few free Hottentots still remain here, who live in their ancient manner; but who are miserable wretches, having hardly any stock of cattle.

29th, To Kromme Rivier (that is, Crooked River) a long, marshy vale, which lies much lower than the former, and is bounded by a continuation of the abovementioned mountains.

30th,

30th, To Effe Bofch, where we encamped that night in the open fields, clear of the woods, for fear of the lions.

Dec. 1ft, We entered a fine level country, bordering on the Eaftern Ocean, leaving behind us the chain of mountains before mentioned, which runs obliquely acrofs the country from the Atlantic to the Indian Ocean. At night we came to Zee-Koe Rivier, or Sea-Cow River, fo called, erroneoufly, from the *hippopotamus amphibius*, which formerly inhabited it, but is now almoft extirpated. We refted here eight days; in which time we ranged the adjacent woods and fields, where we greatly increafed our collection. The river was frequented by a variety of water-fowl which afforded us good fport: there were numbers of the *phenocopterus ruber*, *pelicanus onocrotalus*, with many others, which we could not clafs, being unprovided, as I faid, with books of Ornithology. We lodged at the houfe of JACOB KOCK, an old German, who ufed us with great civility. He had built a handfome houfe, made gardens and vineyards, poffeffed numerous herds of cattle, and had upwards of a hundred Hottentots in his fervice, whom he employed in taking care of them. The face of the country changes greatly, being open, plain, and covered with verdure, extending many miles along the fea-coaft, containing feveral tribes of Hottentots. The rivers formerly abounded with the *hippopotamus amphibius*; but fince the Dutch inhabited thefe parts, they have almoft deftroyed them. They fhoot them

them for their fleſh, which they eſteem equal to pork, their fat being much of the ſame quality. The manner in which the Hottentots catch theſe animals is as follows: the banks of the rivers, as I have already obſerved, are covered with almoſt impenetrable woods; theſe animals in the day time lodge themſelves in the deepeſt places of the river, and when night comes, make excurſions into the adjacent fields to graze, taking their courſe through paths, which they have made in the woods. In theſe paths the Hottentots dig large pits, which they cover over with boughs of trees and graſs; then hunting them out of the fields, the animals make full-ſpeed towards the river, and fall into theſe pits; from whence they are unable to get out, on account of their great weight, and then the men come up with their lances and kill them. We found here a new palm, of the pith of which the Dutchman told us the Hottentots make bread; but we could get no ſatisfactory account of their method of making it. We obſerved two ſpecies; one about a foot and a half diameter in the ſtem, and about twelve feet high, with entire leaves; they appeared to be very old, and ſeldom bore fruit. The other ſort had no ſtem, with the leaves a little ſerrated, and lying flat on the ground, which produced a large conical fructification about eighteen inches long, and a foot or more in circumference; ſquamoſe, and under each of the *ſquamæ*, is an oval nut, about the ſize of a cheſnut, of a beautiful red colour, but inſipid taſte. The male plant is ſimilar

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to the female, only not producing fruit, but bearing a *ſtrobulus*, and containing the *pollen*, or male-duſt, in ſmall cells underneath its *ſquamæ*. In the woods here we found the *euphorbia antiquorum* forty feet high. The inhabitants obſerve, that the honey found near theſe trees is unwholeſome. Being ſtill determined to continue our journey about 150 miles further, directing our courſe towards the middle of the country, and to return to the Cape another way; I furniſhed myſelf with a ſet of freſh oxen and a fortnight's proviſion; and Mr. ROCK gave us one of his ſons for a guide and to ſerve us as interpreter, he being a perfect maſter of the Hottentot language.

9th, We took leave of our hoſpitable friend, and departing towards the evening, we ſtopped that night at the houſe of JACOB VAN RENNEN, a wealthy grazier: this was the laſt Dutch place in this part of the country.* From hence we travelled through a rugged hilly country, covered with thick coppices of evergreen trees; but the way was ſo rough that our waggons were almoſt ſhaken to pieces. Towards noon we croſſed Camtour's River, where we reſted during the heat of the day, and amuſed ourſelves in the woods along its banks, which were extremely pleaſant: the river is broad and deep in many places. The woods are frequented by elephants, buffaloes, and lions; and the deepeſt parts of the river by the *hippopotami*. We found many new plants here, notwithſtanding our ſtay was ſo ſhort. In the afternoon we advanced

through

through a woody country, where we obſerved numbers of butterflies, which appeared like thoſe of India; but from the thickneſs of the woods we could not procure a ſingle ſpecimen. At night we came to Lory's River, ſo called from a ſpecies of parrot, which is found here. We were viſited by ſeveral Hottentots, who came out of the woods armed with lances, but behaved very obligingly, and ſlept by our fire all night; and we at the ſame time entertained them with tobacco, of which they were exceedingly fond.

11th, We travelled over a pleaſant country, diverſified with ſmooth green hills, interſperſed with evergreens, and ſtocked with numerous flocks of the *capra dorcas* of LINNÆUS, *equus zebra*, and *camelus ſtruthio*; which, together with the fine diſpoſition of the woods and groves, could not but charm us, who, for upwards of three months, had been climbing rugged mountains, and croſſing ſultry deſarts. In the evening we came to Van Staad's Rivier, where we remained all night, and were viſited by ſeveral Hottentots, who brought us milk in baſkets made of fine reeds, which they weave ſo cloſe that they hold any liquid.

12th, We croſſed Van Staad's Rivier, where there is a large Kraal, or Hottentot village, containing upwards of 200 inhabitants, who are poſſeſſed of great herds of bullocks, but of no ſheep. Theſe Hottentots were remarkably well-ſhaped, and ſtouter made than any other Hottentots I have yet ſeen. They are alſo very bold in

encountering wild beafts, particularly the lion, which often attacks their folds, and makes great havock. When this happens, all the young men of the Kraal go in purfuit of him, directed by fmall dogs, who follow his fcent: as foon as they difcover him in the bufhes, they irritate him, till he fprings out with fury and attacks them; when being all armed with haffagays, they often throw twenty or thirty into his body at once; but it is common to lofe a man or two in fuch attacks. Thefe Hottentots were all cloathed in *croffes*, or mantles, made of the hides of oxen, which they drefs in a particular manner, making them as pliant as a piece of cloth: they wore the hairy fide outwards. Their breaft, belly, and thighs, were naked, except being croffed by a number of leathern ftraps round their middle. They had no other covering for their private parts, than a muzzle of leather exactly covering the extremity of the *penis*, and fufpended by a leathern thong from their girdle, which was commonly ornamented with brafs rings. Some had the fkin of a fteenbock hung over their breaft, with the fkin of its fore legs and hoofs behind, which they look upon as a great ornament; others had a buffalo's tail, faftened to a girdle which was tied round the thigh; others a porcupine's quill ftuck through each ear; others had plates of brafs of fix inches fquare faftened to their hair, hanging on each fide of their head; others large ivory rings round their arms, with feveral other ridiculous fancies too tedious to mention. The women were dreffed almoft in the fame tafte, except that a great number of fmall thongs of leather, fufpended from.

from their girdle, reached down to their knees, and in ſome meaſure concealed their nakedneſs. They have captains or chiefs over each Kraal, who claim the greateſt part of the herds; the others ſeem only to be ſervants, though they have every thing in common, and pay little reſpect to their ſuperiors. Theſe Hottentots are called Gunaquas, but were mixed with another people whom the Dutch call Caffers, who border upon Terra de Natal. They were all armed with haſſaguays, of which every one had eight or ten in his left hand. We found here the true Cape jaſſemine, or *gardenia ſtellata*, and the coral tree, *erethrina corallodendron*. The climate here differs much from that of the Cape. They have no S.E. wind, which is ſo troubleſome there; their ſtrongeſt wind is from the S.W. They ſeldom have rain in ſummer, though often thunder and lightning; the clouds being attracted by the lofty mountains are ſpent in ſhowers before they reach the plain.

13th, 14th, We made but very ſhort ſtages, employing our time in collecting plants, all of which were new. The buffalo is numerous in this country: it is a fierce animal, and larger than the biggeſt of our Engliſh oxen. In the day-time they retire to the woods, which renders it very dangerous to botanize there. We here ſaw two lions for the firſt time, at about 4 or 500 yards diſtance; but they took no notice of us, keeping their eyes upon a clump of the *capra dorcas*, which were feeding at ſome diſtance from them. We ſhot two of the buffaloes which proved good eating.

15th, To Zwart Kop's Rivier, where we reſted all night.

16th, To Zwart Kop's Salt-pan, where we remained moſt part of that day. This Salt-pan is a lake ſeveral miles diſtant from the ſea, and upon an eminence. In the rainy ſeaſon it is filled with freſh water, which, by the ſaltneſs of the ground, ſoon becomes ſtrongly impregnated with ſaline particles; and when the ſummer's heat exhales the freſh water, the bottom of the lake is covered with a cruſt of pure ſalt two or three feet thick. The lake is about three miles round, and ſurrounded by a riſing ground, covered with a great variety of curious ſhrubs, many of which proved new. Here we found ſeveral ſingular inſects, and among many others the *gryllus* and *cimex*.

17th, We travelled through a miſerable parched country, covered with ſhrubs and ſucculent plants of various kinds; but the graſs was entirely burnt up by the heat of the Sun. We ſaw numbers of wild animals, and in particular a variety of the Zebra, called by the Hottentots Opeagha. We alſo obſerved the print and dung of elephants and lions. At noon we came to Sunday's River, where we reſted a few hours, and conſulted with our guide, whom we took from the laſt Dutch place, about proceeding on our journey. But both he and our ſervants refuſed to advance further; telling us, we were now on the borders of a powerful nation of Hottentots, called Caffers; who, they ſaid, would kill us, were it only to get the iron belonging to our waggons. In conſequence of theſe remonſtrances, and the bad ſtate our carriages

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were in, being ready to drop to pieces, and many of our oxen ſick, we, with much reluctance, conſented to return the ſame way we came.

20th, We arrived again at Sea-cow River, where we rapaired our waggons.

24th, 28th, We proceeded homewards through Kromme Rivier and part of Lange Kloof; but being informed there was a hot bath about a day's journey to the Northward, we determined to ſee it, leaving our waggons and ſervants in Lange Kloof.

29th, Towards the evening we croſſed the ridge of mountains on the North-ſide of Lange Kloof, and at night came to a ſolitary cottage belonging to a Dutchman, where we found ſeveral Dutch people, who were going next day to the hot bath, to uſe the water. We were glad of their company, and travelled over the drieſt country I ever beheld. The plains were covered with looſe ſtones, and not a blade of graſs to be ſeen; but we found many rare ſpecies of *craſſula*, *meſembryanthemum*, and other ſucculent plants. In ſome places not a drop of water was to be found within thirty miles circuit. We could of courſe expect to ſee but few animals; thoſe were the *capra dorcas*, *equus zebra*, *kocdocs*, and ſpringbocks.

30th, At night we arrived at the hot bath, which is ſituated at the foot of a ridge of dry mountains: the water is very hot, and taſtes ſtrongly of iron. There is a Dutch ſettlement about 300 yards from the fountain, where they float their gardens every night with

the water, which at that diſtance is ſtill ſmoaking. By this means they have all kind of garden vegetables in the greateſt perfection. Next morning we went up to the top of this ridge of mountains, which appeared like a maſs of rocks heaped one on top of another, where we had an extenſive view of the country, which appeared horrible, every thing being parched up, and even the beds of the largeſt rivers entirely dry. We found here a ſpecies of heath remarkable for having its branches and leaves all covered with a fine hoary down or nap, which we thought ſingular in that *genus*: we called it *erica tomentoſa*.

Jan. 1ſt, We returned to Lange Kloof, and next day overtook our waggons; but many of our oxen were ſick, having caught a diſeaſe which rages there amongſt the horned cattle in ſummer, and ſo affects their hoofs that they often drop off, and great numbers die. This diſeaſe proves detrimental to the Dutch peaſants, who live 5 or 600 miles in the country, when they make a journey to the Cape. Their oxen are often ſeized with it in the middle of a deſart, and ſometimes muſt remain there for a month till they recover. This makes their journies to the Cape long and diſagreeable, eſpecially as they are obliged to take with them their wives and children, for fear of their being murdered by the Hottentots in their abſence.

3d, We came to Great Thorney River, where we again parted with our waggons, in order to examine a large tract of Carro, where it was improper to take our

oxen on account of the ſcarcity of water. Late in the afternoon we came to a peaſant's houſe, who informed us, he had a neighbour about four hours ride from his place, by whom we ſhould be kindly received, and who would further direct us on our journey. After having put us in the road, and given us ſome directions, he parted with us, and we purſued our journey till ſun-ſet, but found no habitation. We therefore concluded, that we had certainly loſt our way, and returned ſome miles back, where we found a road which branched off another way. In this path we continued till one o'clock in the morning, having got into a diſmal valley, incloſed on each ſide with rugged precipices: at laſt we found ourſelves in the middle of a thicket of thorn trees *(mimoſa nilotica)* where we unſaddled our horſes and kindled a fire. We paſſed the night with little comfort, having eaten nothing all that day; but to our great ſatisfaction we heard the murmuring of a ſtream, which we went in ſearch of, and found good water: our concern, however, was ſtill great for our poor horſes that had nothing to eat. We ſpent the night in gathering wood and keeping our fire up till day-light, when I climbed up a high precipice, and viewed the country. Here I collected ſeveral curious plants, *geranium ſpinoſum, ſtapelia euphorbioides*; and upon my return, we mounted our horſes, and directed our courſe towards the high mountains, where we expected to find ſome relief, but were diſappointed; for after being parched up with inſupportable heat, we met not with a drop of water to quench our thirſt

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during the whole day's journey. But towards the evening we happily diſcovered a houſe, where we were kindly entertained, and the next morning overtook our waggons in Hartwig's Kloof; but our oxen were in a bad ſtate, and one of them was quite unfit for ſervice We continued our journey without any other remarkable event, except that of loſing more of our oxen by the above mentioned diſeaſe.

12th, Came to Buffels Tagt River, where we reſted ſeveral days, ranging the adjacent woods, where we found many curious trees in bloom.

29th, We arrived at the Cape Town, after a journey of four months and fourteen days.

THIRD JOURNEY.

R. Feb. 29, 1776. SEPT. 26, 1774, I ſet out from the Cape Town, but by the badneſs of the weather was obliged to ſtay all night at the Salt River, about two miles from the town, where there is a wine-houſe. I had in company only two ſervants, for driving my waggon and taking care of my oxen and horſe.

27th, The morning being fine we travelled through the great ſandy plain (lying between the Cape Town and Hottentots Holland Mountains) great part of which was under water. In the afternoon we had heavy rain, when

we croſſed the Eerſte Rivier, and lodged all night at a farm-houſe under thoſe mountains, were we found the whole country enamelled with flowers.

28th, 30th, The weather began to grow more pleaſant, the Sun ſhining out with force; but ſudden heavy ſhowers much retarded our journey, confining us to ſhort ſtages along the foot of the Stelleboſch Mountains.

Oct. 1ſt, To Draaken Steen.

2d, To Paarle Kerk, where I was joined by Dr. THUNBERG.

4th, We went up to the top of the Paarle Mountain, where we added greatly to our collection.

5th, To Paarde Berg (Horſe Mountain).

6th, We mounted to the top of Paarde Berg, where we found a treaſure of new plants, which we had not ſeen before, and on the top had an extenſive view of the adjacent country, which is level, and has but a barren appearance; yet contains ſeveral rich plantations, producing abundance of corn and wine; and the peaſants live luxuriouſly. Their plantations lie all around the foot of this mountain, which yields a number of fine rivulets, without which this country would be uninhabited.

7th, We directed our courſe Northward, through a level country covered with low ſhrubs; but it being now ſpring, it was every where decorated with flowers of the greateſt beauty, every hour's march producing new charms. At night we arrived at the foot of a mountain called Van Riebeck's Caſteel. There we lodged at Mr. DRAYER'S, a wealthy

wealthy farmer, who treated us in the moſt friendly manner, and begged that we would favour him with our company for a month, which ſhould not coſt us a farthing.

9th, We went up to the top of Riebeck's Caſteel, which is very high, and on the North ſide inacceſſible. It is about four or five miles long, and very narrow on the top; we collected here many remarkable new plants, in particular a hyacinth, with flowers of a pale gold colour.

10th, We came to the Berg Rivier, which was then impaſſable by reaſon of the late rain.

12th, With ſome difficulty we tranſported, in a large boat, our waggons and baggage to the oppoſite ſide, and afterwards obliged our oxen to ſwim over. From thence we proceeded through a barren uninhabited country; conſequently were obliged to content ourſelves with the ſhelter of a large *leucodendron*, that protected us from the S.E. wind, which at this ſeaſon ſometimes blows cold.

13th, We arrived at the foot of a mountain called Piquet Berg, lying direct North from the Cape Town, being a particular place of obſervation of the Abbé DE LA CAILLE, when he meaſured a degree on the meridian in the year 1750. All around the mountain the ſoil is ſandy, but furniſhed with a great variety of beautiful plants, eſpecially *aſpalathi*.

15th, We mounted the Piquet Berg, which is very high but eaſy of aſcent. On the top are fine plains covered

vered with excellent verdure, which are of great ſervice to the peaſants, who ſend up their oxen during the ſummer ſeaſon. We ſaw here ſeveral zebras and two colts, but they were very ſhy.

18th, We came to Verloore Valley, which begins on the N.W. ſide of the Piquet Berg. It is a narrow extent of marſhy ground, incloſed by hills on each ſide, with a ſmall river, frequented by a variety of water fowl, which afforded good ſport. Towards the ſea, the river increaſes in breadth, in many places upwards of a mile, and is very deep; there we ſaw hundreds of pelicans and wild geeſe, which kept the middle of the river; but we ſhot ſeveral wild ducks and water hens, which ſwam among the reeds along the ſide of it.

23d, We arrived at the mouth of the Verloore River, where it is diſcharged into the ſea; but found the coaſt barren, conſiſting of ſandy hills, ſo looſe that our horſes were ſometimes up to their bellies, which made our journey very fatiguing.

23d, We left the ſhore on our left hand, and directed our courſe Northward towards the mouth of the Olyfant's Rivier. The heat became now great, which the whiteneſs of the ſand ſtill increaſed, and obliged us to travel late in the evening and early in the morning, reſting in the middle of the day. It was alſo not a little fatiguing to travel here on horſeback, the mole-caſts being ſo deep that the horſes fell up to their ſhoulders every ſix or ſeven minutes. This animal is by the Dutch called Land-moll, but differs ſo much from the European mole, that

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it does not belong to the ſame claſs of animals, but is intirely new. It feeds upon the roots of *ixiæ*, *gladioli*, *antholyzæ*, and *irides*, often grows to the ſize of a rabbit, and by ſome is eſteemed good eating. There is another ſpecies of the animal, called by the Dutch Bles-moll, which inhabits the hard ground; but ſeldom exceeds the ſize of the common European mole. This country is furniſhed with a great variety of elegant ſhrubs; viz. *eniſtæ*, *partia*, and *aſpalathi*. At night we came to Lange Valley, where we took up our lodging in a deſolate place, the inhabitants being all removed; for this is only their winter reſidence, when the water is freſh, which had now began to be brackiſh.

24th, We ſet out early in the morning, expecting to find a river or fountain, where we could reſt during the heat of the day; but, to our no ſmall diſappointment, we travelled till noon without finding any: our oxen were ſo hot that their tongues hung out of their mouths. About one o'clock we ſaw a lake of water at ſome diſtance, but on our arrival our horſes refuſed to drink: we diſmounted, and found it to be a ſalt lake. In the evening we came to a fountain of excellent water, where we ſpent the night with great comfort. Next morning we were viſited by a peaſant going to the Cape; who told us, he had been attacked in the night by a lion, which made a ſpring at his Hottentot who led the oxen, but happily miſſed him. He admoniſhed us to be expeditious, and get to ſome habitation that night, otherwiſe we might expect a viſit from him.

 25th,

25th, At noon we proceeded on our journey, the road continuing ſtill very bad; and in paſſing along we ſaw the prints of the lion's feet in ſeveral places. At night we came to Olyfant's Rivier, where we found a Dutch habitation; there we reſted ſeveral days, being treated with great hoſpitality. This country abounds with game. They have two kinds of partridges, which are exceedingly plentiful and eaſy to ſhoot; and a perſon cannot walk ten paces without raiſing a brace of quails. Their hares are of an extraordinary ſize, but differ little otherwiſe in character from thoſe of Europe. We hunted every day, and by the aſſiſtance of the peaſant's ſon, who was an excellent markſman, never failed to come home loaden. The ſteril appearance of this country exceeds all imagination: wherever one caſts his eyes, he ſees nothing but naked hills, without a blade of graſs, only ſmall ſucculent plants. The ſoil is a red binding loam, intermixed with a kind of rotten *ſchiſtus* or ſlate. Next morning we traverſed the adjacent hills, and were ſurprized to find all the plants entirely new to us. They were the greateſt part of the ſucculent kind; viz. *meſembryanthemum*, *euphorbia*, and *ſtapelia*, of which we found many new ſpecies. The peaſant told us, that in winter the hills were painted with all kind of colours; and ſaid, it grieved him often, that no perſon of knowledge in botany had ever had an opportunity of ſeeing his country in the flowery ſeaſon. We expreſſed great ſurprize at ſeeing ſuch large flocks of ſheep as he was poſſeſſed of ſubſiſt in ſuch a deſart; on which he obſerved,

that their ſheep never ate any graſs, only ſucculent plants, and all ſorts of ſhrubs; many of which were aromatic, and gave their fleſh an excellent flavour. Next day I paſſed through a large flock of ſheep, where I ſaw them devouring the juicy leaves of *meſembryanthemum*, *ſtapelia*, *cotyledon*, and even the green ſeed veſſels of *euphorbia*; by eating ſuch plants they require little water, eſpecially in winter.

30th, We were employed in unloading our waggons, and tranſporting our baggage acroſs the river in a ſmall boat; and afterwards drove over our oxen with the empty waggons, which were almoſt overſet in the middle of it. The river is about forty or fifty yards broad, and in ſome places very deep. The borders are covered with the *mimoſa nilotica*, which forms a thick impenetrable wood. We were about a day's journey from the mouth of this river, where are ſtill ſome elephants remaining, the country being very wild and uninhabited. We had now the great Carro to paſs; a deſart of three days journey, where no freſh water, and only three pits of brackiſh water, enough to preſerve the lives of our cattle, were to be found. Theſe pits are at ſome diſtance from the road, which makes it very difficult for ſtrangers to find them. But while we were ſeriouſly conſidering theſe approaching difficulties, thinking, if we ſhould miſs the pits, we ſhould probably periſh in this inhoſpitable deſart, to our great joy we were overtaken by a Boor, with his wife and children, who were going the ſame road; but he having a freſh team of horſes, we could not keep up with him.

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However, he directed us in the way; and told us, he would tye a piece of white cloth on a branch of a tree, where he knew there was water; but desired us not to go to those places without fire-arms, as there was commonly a lion lurking near them; who knowing that all the animals must come there to drink, he seldom failed to seize his prey. At night we overtook our fellow traveller, who had taken up his lodging on a bare eminence, without a bush to shelter him; though at some distance there was a small wood of *mimosa* trees along the banks of a river that was then dry, which we thought much preferable to his situation. But he told us, it was much more dangerous on account of wild beasts; and that there often fell such sudden showers in the mountains, that people who had lodged by the rivers, had, with their waggons and oxen, been carried away in the night while they lay asleep. He left us early next morning, but we were obliged to stay till noon to let our oxen feed, and then went on until sun-set; but unhappily found no water, which mortified us much, having a long day's journey to the next watering place. All next day we travelled over this thirsty land, where we suffered from the heat of the Sun and want of water; but our sufferings were still aggravated when we thought on our poor animals, who often lay down in the yoke during the heat of the day. This desart is extensive; being bounded on the N. and N.E. by a chain of flat mountains, called Bockland's Bergen (Bockland's Mountains) and on the W. and N.W. by the Atlantic Ocean.

It

It is uninhabitable in ſummer; but in winter, or during the rainy ſeaſon, the Bockland people come down with their herds, which by feeding upon ſucculent ſhrubs, that are very ſalt, in a ſhort time grow remarkably fat. There ſtill remains a great treaſure of new plants in this country, eſpecially of the ſucculent kind, which cannot be preſerved but by having good figures and deſcriptions of them made on the ſpot; which might be eaſily accompliſhed in the rainy ſeaſon, when there is plenty of freſh water every where. But at this ſeaſon of the year, we were obliged to make the greateſt expedition to ſave the lives of our cattle, only collecting what we found growing along the road ſide, which amounted to above 100 plants, never before deſcribed. Towards the evening we arrived at the foot of Bockland's Berg, where we paſſed the night by a penurious ſtream of freſh water, but which yielded us no ſmall comfort.

Nov. 2d, The peaſant who had paſſed us in the Carro, as ſoon as he arrived at home, immediately ſent two team of freſh oxen to help us up the mountain, our own being much weakened by the heavy roads. In the cool of the afternoon we aſcended by a winding road, which was ſo very rugged and ſteep, that it took five hottentots with ropes made faſt to the waggon to keep it from overturning. The face of the mountain conſiſts intirely of ſcattered rock, being acceſſible only in this place, and is overgrown with a great variety of large woody plants, moſt of which were new. We found a new ſpecies of aloe here, called by the Dutch Koker Boom,

Boom, of which the Hottentots make quivers to hold their arrows; it being of a ſoft fibrous conſiſtence, which they can eaſily cut out, leaving only the bark, which is hard and durable. Theſe trees were about twelve feet high, with a ſtrait ſmooth trunk, about ten inches or a foot diameter and five or ſix feet in length, which divided into two branches; and thoſe were again ſub-divided into two more branches, which terminated in a bunch of thick ſucculent leaves ſurrounding the ſtem, ſpear-ſhaped, entire, without ſpines, and hanging down like the leaves of *dracæna draco*. We did not ſee it in flower, but by the above characters took it for a new ſpecies, and called it *aloe dichotoma*. We gained the top of the mountain, and entered into Bockland, which is extended along the ſummit for many miles. It is pretty level, but very rocky. We enjoyed a pure cool air, it being ſeveral degrees colder here than in the Carro. Bockland lies nearly in a Northern direction from the Cape, and at the diſtance of about 220 miles. It was called Bockland on account of the amazing quantity of ſpring bucks which were formerly found there; but ſince this country has been inhabited by Europeans, it has ceaſed to be the ſettled reſidence; at leaſt, the number of thoſe which conſtantly remain in it is very inconſiderable. It generally happens, however, once in ſeven or eight years, that flocks of many hundred thouſands come out of the interior parts of Africa, ſpreading over the whole country, and not leaving a blade of graſs or a ſhrub. The peaſants are then obliged to guard their corn fields night and day, otherwiſe thoſe animals would cauſe a famine wherever they paſſed.

paſſed. It ſeems probable, by the accounts of theſe extraordinary emigrations, that their natural habitation is in the interior parts of Terra de Natal; and that they are forced Southwards by dry ſeaſons, which happen ſometimes in thoſe regions to ſuch a degree, that not a drop of rain will fall for two or three years together. Theſe great flocks are ſaid to be always attended by lions; and it is obſerved, where a lion is, there is a large open ſpace. We ſaw ſeveral flocks, but not exceeding twenty in each. We met a party of Dutchmen, who had been about 150 miles to the Northward of Bockland, deſtroying the Boſchman Hottentots. They informed us, they had ſeen great flocks of the ſpring bucks; but there happening much rain, which had recovered the graſs and vegetation, they had been obſerved to change their courſe, and return to the interior parts of the country.

3d, 4th, We continued our journey along this elevated tract; having on our right hand, or South-ſide, the precipice, which is inacceſſible; and on the North-ſide, a deſolate hilly country, inhabited by a few wandering tribes of the Boſchman Hottentots. At night we came to the place of our benefactor, whoſe name was KLAAS LOSPER; he was a very opulent man in thoſe parts, having upwards of 12000 ſheep and 3000 bullocks. Moſt of the plants that we collected here were new; and, I believe, many more remain, this having been the dry ſeaſon, when moſt of the flowers were gone.

6th, We directed our courſe Northward, through a dry, barren country, called Hantum; and on the 10th

came

came to the laſt Dutch habitation on this ſide of the country. As we paſſed along we found many new plants growing near the banks of rivers, which were then quite dry; but the ſoil conſiſted of nothing but rotten rock. The hills were of the ſame ſubſtance, all of a conical figure, and entirely covered with pieces of rock, about the ſize of a man's fiſt. We continued ſeveral days at this habitation, where we were well entertained. They had excellent bread, good mutton, butter and milk, but no kind of ſtrong liquors. We made ſeveral enquiries about the country lying to the Northward; and were told, that it had been formerly inhabited by Europeans near a hundred miles further, who at firſt had greatly increaſed their herds; but that ſome dry ſeaſons coming on afterwards, they had been forced to return: the country therefore was ſuppoſed to be uninhabited, except by the wandering Hottentots, who ſeldom ſtay above a month in a place. This place is about 350 Engliſh miles North from the Cape of Good Hope. We now changed our courſe, going directly S.E. through an uninhabited country much like the former, ſurrounded by high mountains, flat on the tops, and forming what the peaſants call Table Mountains. I never ſaw the ſmalleſt rivulet or fountain iſſuing from them; all the water that we found being that which was left ſtagnant in the deepeſt parts of the rivers, that are formed by the rain in the winter ſeaſon, which rivers, towards Midſummer, in other places become entirely dry.

14th, To Rhinoceros Rivier. Here we ſaw great herds of zebras, and were informed by three Dutchmen, who paſſed us on horſeback, that this place was frequented

quented by a large lion; and, as a proof, they ſhewed us a zebra, which he had lately killed; aſſuring us, if we ſtayed all night there, he would pay us a viſit. We travelled about ten miles further, and at night ſaw a flock of ſheep and ſome bullocks, which greatly animated us, expecting to find ſome habitation where we might ſhelter ourſelves during the night; but, when we came to the place where the ſheep were, we found a Dutchman with his wife and ſeveral young children ſitting under the ſhelter of ſome buſhes, which they had formed into an alcove, to ſkreen them from the heat of the Sun. We ſtayed here all night, and the man aſked us to ſup with them; which we did, and made them a preſent of ſome tea and tobacco, which they thankfully received; and the next day the huſband ſaddled his horſe, rode ſix or ſeven miles with us, and gave us very good directions how to proceed in our intended courſe.

16th, We aſcended a flat chain of mountains, called Rogge Velds Berg, where we found the road extremely rugged. Rogge Veld extends along the ſummit of a high ridge of mountains, running obliquely acroſs the country for ſeveral hundred miles. It is very arid, except in ſome vallies, where the Dutch peaſants have their habitations; but the general face of the country is rock. The ſoil is a red ochrey loam; it binds very hard in ſummer, and is in moſt places ſalt, which cauſes bad water. There is not a tree in the whole country, unleſs we ſhould ſo call a few miſerable ſhrubs, and of theſe the largeſt not

 exceeding

exceeding two feet in height. The air is very ſharp, and in winter they have froſt and ſnow for ſeveral months, which obliges the Boors to remove, with all their flocks and herds, down to the Carro, or lower deſarts, where they ſpend the winter; and at that time have plenty of freſh water, and all the ſhrubs green, which afford food for their cattle. They remove down in the beginning of May, when they have ſown their corn, and return about the latter end of October, when the low country becomes parched, and the water turns ſalt, or is entirely dried up. All the game and ferocious animals obſerve the ſame removes. The ancient inhabitants of this country, called by the Dutch Boſchmenſchen, are a ſavage people and very thieviſh; often carrying off 700 ſheep at a time, and killing their ſhepherds. They uſe bows and arrows, and poiſon the arrows with the venom of ſerpents mixed with the juice of a ſpecies of *euphorbia*, which we had no opportunity of ſeeing. Theſe Hottentots have neither flocks or herds, nor any fixed habitation, nor even ſkins to cover them; but live in the cavities of rocks, like baboons. Their common food is roots of plants, many of which we have not been able to diſcover. They eat ſnakes, lizards, ſcorpions, and all kind of reptiles. There is a caterpillar which produces a very large moth, and is found commonly on the *mimoſa nilotica*. Theſe are found in great plenty, often ſtripping the trees of all their leaves, and of them the Hottentots make many a delicious meal. They alſo eat the

eggs of a large ſpecies of ant, which they dig out of the ground in great quantities, waſhing them in water, and afterwards boiling them. They are commonly called Hottentot's rice. This is an excellent country for ſheep; but the inhabitants breed few oxen, and thoſe only for their own uſe. We found few plants here; but thoſe we found were all new. I did not ſee an *erica* or *protea* in the whole country.

22d, The ground was white with froſt, and the wind ſharp. At firſt we propoſed to continue our journey along the top of theſe mountains to the N.E. extremity; but our waggons were ſo ſhaken by the ruggedneſs of the road, and our horſes and oxen ſo tenderfooted, that they became unſerviceable, and we were obliged to drive them looſe a great part of the way home.

Dec. 2d, We thought of deſcending the mountain, and directing our courſe to the Cape; but it blew a violent ſtorm, and was extremely cold. The next morning the ground was white with froſt, and there was ice upon the pools as thick as a crown piece. This alarmed the peaſants, their wheat being then in bloſſom, which they expected would be entirely deſtroyed: a circumſtance that often happens in this country.

3d, We were furniſhed with freſh oxen, and ſeveral Hottentots, who, with long thongs of leather fixed to the upper part of our waggons, kept them from overturning, while we were obliged to make both the hind

wheels faſt with an iron chain to retard their motion. After two hours and a half employed in hard labour, ſometimes pulling on one ſide, ſometimes on the other, and ſometimes all obliged to hang on with our whole ſtrength behind the waggon, to keep it from running over the oxen, we arrived at the foot of the mountain, where we found the heat more troubleſome than the cold had been on the top. We now entered a large diviſion of the Carro which lies along the foot of the Rogge Veld's Mountains, being a deſart of four days journey, with no more than three pits of brackiſh water to be found in all that extent, which was at this ſeaſon forſaken by every living creature; but in winter it is the habitation of the Rogge Veld Boors, as I obſerved before.

5th, To Unlucky River, called ſo from a man having been there formerly devoured by a lion. We remained here a day to reſt our oxen, having found a pit with brackiſh water, and ſome reeds, which the oxen devoured with greedineſs.

8th, About eleven o'clock at night we got clear of the deſart, and arrived at the foot of the Bocke Velde mountains, where we lodged by a rivulet of pure freſh water; and we ſpent the remainder of that night and part of next day in great luxury.

11th, To Verkeerde Valley, where we reſted three days, having found good paſture for our oxen, and a large lake of freſh water, well ſtocked with water-fowl. We

lived on wild ducks and ſnipes, though the fields abounded alſo with korhaans (a kind of buſtard), partridges, hares, &c. and great flocks of oſtriches.

15th, To Hexen Rivier, which runs along a narrow paſſage through the great chain of mountains, between Rood Land and Zwellendam. This valley is incloſed on each ſide with impaſſable mountains, whoſe tops were ſtill covered with ſnow. There are ſeveral very genteel habitations in it, where we got ſome wine and excellent fruit. We found many rare plants on the ſides of theſe lofty mountains; and, I believe, there ſtill remain many more entirely unknown to us.

18th, To Breede Rivier (Broad River).

22d, To Rood Land.

26th, To Paarde Berg.

28th, To the Cape Town.

METEOROLOGICAL JOURNAL

KEPT AT THE HOUSE OF

THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1775.

	Time. H.	Time. M	Therm without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Jan. 1	8	0	34,0	35,5	29,75		NW	1	Fine.
	2	0	41,0	37,0	29,71		NW	1	Fair.
2	8	0	29,0	34,5	30,17	0,062	W	1	Fine.
	2	0	33,0	34,0	30,19		W by N	1	Fine.
3	8	0	39,[illegible]	36,5	30,02	0,114	W by N	0	Fog.
	2	0	43,5	37,5	29,91		WNW	1	Fine.
4	8	0	39,5	40,5	29,82	0,016	W	2	Fair.
	2	0	44,0	41,5	29,87		W	1	Fine.
5	8	20	40,0	41,5	30,02		W	1	Fair.
	2	0	45,0	43,0	29,98		WNW	1	Fair.
6	8	0	45,5	44,5	29,93		WNW	1	Fine.
	2	30	48,0	46,0	30,06		WNW	1	Fine.
7	8	0	46,0	45,0	30,06		WNW	1	Fair.
	2	0	50,5	47,0	30,00		WNW	1	Fine.
8	8	0	52,0	49,0	29,92		WNW	2	Cloudy.
	2	0	54,0	50,0	29,91		WNW	1	Fair.
9	8	0	48,5	51,0	30,18	0,034	WNW	1	Cloudy.
	2	10	52,5	52,0	30,21		WNW	1	Fair.
10	8	0	48,5	51,5	30,18	0,010	WNW	1	Cloudy.
	2	0	49,5	51,5	30,11		WNW	1	Fair.
11	8	0	47,0	50,0	29,86		WNW	2	Rain.
	2	0	48,5	50,0	29,73		WNW	2	Rain.
12	8	0	41,0	48,5	29,69	0,086	WNW	1	Fine.
	2	0	47,0	49,5	29,69			1	Fine.
13	8	0	40,5	45,5	29,74	0,030		1	Fine.
	2	30	45,5	47,0	29,75			1	Fine.
14	8	30	45,5	45,0	29,75			1	Cloudy.
	2	10	49,0	46,5	29,72			1	Rain.
15	8	30	46,0	47,5	29,77	0,115		1	Rain.
	2	0	47,0	49,0	29,81			1	Cloudy.
16	8	0	41,5	45,5	29,83			2	Fine.
	2	0	47,0	47,0	29,85			1	Fine.

METE-

METEOROLOGICAL JOURNAL for January 1775.

	Time.		Therm. without	Therm. within.	Barom.	Rain	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	42,0	45,5	29,84		ESE	1	Fine.
	2	0	48,5	47,0	29,76		E	1	Fine.
18	8	0	34,0	43,5	30,03	0,055	NNW	1	Fog.
	2	0	37,5	43,5	30,04			1	Fog.
19	8	0	35,0	39,5	29,99		NE	1	Fog.
	2	0	36,0	40,0	29,94		NE	1	Fair.
20	8	0	34,0	38,5	29,88		E	1	Fog.
	2	0	33,0	39,0	29,82		ENE	1	Snow.
21	8	0	38,5	39,0	29,66	0,230	ENE	1	Fog.
	2	0	47,5	44,0	29,59		S by E	1	Fair.
22	8	0	42,0	42,5	29,51		SSE	1	Fine.
	2	0	49,5	45,0	29,47		S by E	1	Cloudy.
23	8	0	44,5	45,5	29,54	0,140	E by N	1	Rain.
	2	0	44,5	46,5	29,63		ENE	1	Fair.
24	8	0	32,0	41,5	30,13	0,204	NE	2	Cloudy.
	2	0	32,0	40,0	30,25		E by N	1	Fine.
25	8	0	25,5	33,5	30,38		E	1	Fine.
	2	15	26,5	34,0	30,285		ESE	1	Fair.
26	8	0	26,5	30,5	29,71		E	1	Snow.
	2	0	40,5	33,5	29,60		WSW	2	Fair.
27	8	0	38,5	36,0	29,78	0,098	SE	1	Fair.
	2	0	46,5	39,0	29,62		SSE	2	Fair.
28	8	0	36,0	40,5	29,62	0,127	SW	1	Fair
	2	10	45,5	42,5	29,70		WSW	1	Fine.
29	8	30	49,0	43,5	29,465	0,323	SSW	1	Fair.
	2	0	52,5	46,0	29,465		SW	1	Fair.
30	8	0	45,0	47,5	29,55		SW	1	Fine.
	2	0	49,5	48,5	29,63		SW	1	Fine.
31	8	0	50,5	49,0	29,32	0,080	SSW	3	Rain.
	2	0	53,5	50,0	29,30		SSW	2	Fair.

METEOROLOGICAL JOURNAL
for February 1775.

	Time. H.	M.	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points	Str	Weather.
Feb. 1	8	0	51,5	50,5	29,14	0,097	SSW	2	Rain.
	2	10	54,0	52,0	28,99		SSW	2	Fair.
2	8	0	43,0	48,5	29,52	0,167	SW	2	Fair.
	2	0	47,0	49,5	29,50		SW	2	Rain.
3	8	0	44,5	48,5	29,60	0,165	SSW	1	Rain.
	2	0	52,0	50,0	29,54		SSW	1	Rain.
4	8	0	53,0	51,5	29,565	0,034	WSW	2	Cloudy.
	2	0	55,0	52,5	29,45		WSW	2	Fair.
5	8	0	39,0	49,0	29,89	0,199	N	1	Fine, windy night.
	2	0	41,0	48,0	30,03		WNW	1	Fine.
6	8	0	39,0	44,0	30,26		SE	1	Cloudy.
	2	0	46,0	45,0	30,24		SE	1	Fair.
7	8	0	48,5	46,5	30,02	0,030	WSW	1	Fair, windy night.
	2	0	53,5	49,0	29,92		SW	2	Fair.
8	8	0	51,0	51,0	29,60	0,049	SW	2	Rain, windy night.
9	8	0	41,0	48,0	29,35	0,225	SW	2	Fair, windy night.
	2	0	48,0	49,0	29,35		SW	1	Fair.
10	8	0	37,0	45,0	29,75	0,165	W	1	Fine.
	2	0	48,5	47,0	29,65		W	1	Fine.
11	8	0	42,0	46,5	29,02	0,092	SW	2	Cloudy, windy night.
	2	0	51,0	47,5	28,98		SW	1	Fair.
12	7	45	37,5	44,5	28,94	0,268	SW	1	Fine.
	2	0	43,5	46,0	28,91		SW	1	Fair.
13	7	45	41,0	44,5	28,89	0,095	SW	1	Fair.
	2	10	46,5	45,5	28,935		SW	1	Fair.
14	8	0	40,0	43,5	29,35	0,019	WNW	1	Fair.
	2	0	45,5	45,0	29,50		WNW	2	Fair.
15	8	0	42,0	43,5	29,225	0,221	WSW	1	Cloudy.
	2	0	48,5	45,0	29,44		NNE	2	Fine.
16	8	0	36,5	42,5	29,75		S	1	Fair.
	2	0	47,0	44,5	29,55				

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METEOROLOGICAL JOURNAL

for February 1775.

	Time.		Therm. without	Therm. within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	38,0	44,5	29,70		WSW	1	Fair.
	2	0	45,5	45,0	29,83		WSW	1	Fine.
18	8	0	35,0	41,5	30,07		WSW	1	Fine.
	2	0	46,5	44,0	30,15		WNN*	1	Fine.
19	8	0	43,5	42,5	30,12	0,018	N by W	1	Fine.
	2	0	48,5	44,5	30,27		N by W	1	Fine.
20	8	0	44,5	43,5	30,37		SSW	1	Fair.
	2	0	53,0	46,5	30,34		SW	2	Fair.
21	8	0	47,5	47,0	30,18	0,010	SW	2	Rain.
	2	0	49,5	49,0	30,17		NNW	2	Rain.
22	8	0	35,5	43,5	30,48	0,016	SSW	1	Fine.
	2	0	48,5	46 0	30,48		WNW	1	Fine.
23	8	0	40,0	43,5	30,30		SSW	1	Fair.
	2	30	48,5	45,5	30,19		SW	1	Fair.
24	8	0	49 0	47,5	30,11	0,058	SW	1	Cloudy.
	2	0	51,5	49,5	30,11		SW	1	Fair.
25	8	0	40,0	47,5	30,04		S	1	Fine.
	2	0	50,0	49,0	30,00		SSW	1	Fine.
26	8	0	37,5	45,5	30,08		NE	1	Fair.
	2	0	52,0	47,5	30,08		SSE	1	Fine.
27	7	45	35,0	44,5	30,14		NNE	0	Fair.
	2	0	53,5	46,5	30,14		SW	1	Fine.
28	8	0	39,0	45,0	30,09		S	1	Fair.
	2	0	54,0	47,0	30,07		SSW	1	Fine.

* So it is in the written journal, probably for WNW. S. HORSLEY.

METEOROLOGICAL JOURNAL, &c.

FOR ONE YEAR,

BEGINNING WITH THE MONTH OF MARCH, 1775.

A Committee of the Society lately appointed to regulate certain matters relating to Meteorology have determined, that the Society's Meteorological Year ſhall, from this time forward, commence with the month of March; to the end, that every ſuch year may conſiſt of one entire ſummer and one entire winter, rather than of the pieces of two different winters with the entire ſummer intervening, as muſt always be the caſe when the Meteorological Year, in conformity to the civil reckoning, begins with the month of January.

S. HORSLEY.

METEOROLOGICAL JOURNAL for 1775.

	Time. H. M.		Therm. without	Therm within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 1	7	0	38,5	46,0	30,00		SSW	1	Fair.
	2	0	56,0	49,5	29,93		S	1	Fine.
2	7	0	43,5	48,5	29,80	0,190	SW	0	Fair.
	2	0	50,5	50,5	29,75		W by S	1	Fine.
3	7	0	39,0	46,5	29,72		SW	1	Fair.
	2	0	44,0	48,0	29,76		W	1	Rain.
4	7	0	38,5	44,0	29,34	0,074	SE	1	Rain.
	2	0	47,0	46,5	29,07		S	1	Rain.
5	7	0	41,0	45,0	29,07	0,095	NE	1	Fair.
	2	0	43,0	46,5	29,23		N	1	Rain.
6	7	0	38,0	44,5	29,54	0,295	SSW	1	Fair.
	2	0	51,0	47,0	29,53		SSW	1	Fair.
7	7	0	38,0	45,5	29,66	0,010	SW	1	Fair.
	2	0	49,0	47,5	29,66		SSW	1	Fine.
8	7	0	35,5	44,0	29,60	0,087	S	1	Fine.
	2	0	46,0	46,0	29,47		WSW	1	Fair.
9	7	0	46,0	44,5	29,75		S by W	1	Cloudy.
	2	0	54,0	47,5	29,70		S by E	2	Fine.
10	7	0	42,0	48,0	29,61	0,169	SW	1	Fair.
	2	0	50,5	49,5	29,71		W	1	Fair.
11	7	0	43,5	47,5	29,865	0,046	SSW	2	Rain.
	2	0	47,5	48,5	29,57		SSW	2	Rain.
12	7	0	46,5	48,5	29,40	0,294	W by S	2	Fair.
	2	0	50,5	50,5	29,43		W	2	Fine.
13	7	0	39,5	46,5	30,02	0,053	W	1	Fair.
	2	0	47,0	48,0	30,27		NE	2	Fair.
14	7	0	33,0	43,0	30,60		SSW	1	Fine.
	2	0	46,0	44,5	30,61		W by S	1	Fine.
15	7	0	33,0	42,0	30,61		WSW	1	Fine.
	2	10	50,0	44,0	30,58		SW	1	Fine.
16	7	0	33,0	42,0	30,52		SSW	1	Fine.
	2	0	49,0	44,5	30,44		NNW	1	Fine.

METEOROLOGICAL JOURNAL for 1775.

	Time. H. M.		Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 17	7	0	35,0	43,0	30,45		SSW	1	Fine.
	2	0	52,0	46,0	30,24		SW	1	Fine.
18	7	0	40,0	44,5	30,02		SW	1	Fair.
	2	0	52,5	47,5	29,88		W by S	1	Fair.
19	7	0	39,5	46,5	29,35	0,189	W by S	1	Fair.
	2	0	45,5	48,0	29,42		N by W	1	Rain.
20	7	0	36,0	44,5	29,78	0,065	NW	1	Fine.
	2	0	51,0	47,0	29,79		W by S	1	Fair.
21	7	0	47,0	48,5	29,85		NW	1	Fair.
	2	0	58,0	52,0	30,04		NW	1	Fine.
22	7	0	47,5	50,0	30,10		SW	1	Fair.
	2	0	56,5	53,0	30,05		SW	1	Fine.
23	7	0	46,0	50,5	30,06		NW	1	Fair.
	2	0	54,5	51,5	30,13		N by W	1	Fine.
24	7	0	40,0	48,5	30,14		WSW	1	Cloudy.
	2	0	57,0	52,0	30,04		W	2	Fair.
25	7	0	48,5	50,5	29,80		S by W	2	Cloudy.
	2	0	54,0	52,5	29,68		S by W	2	Cloudy.
26	7	0	37,5	47,5	29,72	0,093	WSW	2	Fine.
	2	0	42,5	48,0	29,66		NNW	2	Rain.
27	7	0	30,5	41,5	29,79		NW	2	Fine.
	2	0	43,5	43,5	29,80	0,041	SSW	2	Fine.
28	7	0	42,5	42,5	29,28	0,025	SSW	2	Rain.
	2	0	38,0	43,0	29,31		NW	2	Fair.
29	7	0	28,5	38,0	29,30	0,079	W	1	Fine.
	2	0	39,0	39,5	29,38		W by N	1	Fine.
30	7	0	30,0	37,5	29,61	0,022	N	1	Fine.
	2	0	42,0	40,5	29,70		NNW	1	Fine.
31	7	0	29,0	37,0	29,81	0,027	NW	1	Fine.
	2	0	47,0	42,5	29,81		SW	1	Fine.

METE-

METEOROLOGICAL JOURNAL

for 1775.

	Time. H. M.		Therm without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Apr. 1.	7	0	36,5	40,0	29,94		SW	1	Fair.
	3	0	48,0	43,0	30,07		NE	1	Fair.
2	7	20	37,0	42,0	30,19		SbW	0	Fair.
	2	0	55,0	45,0	30,18		WSW	1	Fine.
3	7	0	38,5	44,5	30,18		WSW	0	Fine.
	2	0	54,5	49,5	30,13		SW	1	Fine.
4	7	0	46,0	48,0	30,00		S by W	1	Fine.
	2	0	53,0	50,0	30,00		W	1	Rain.
5	7	0	39,5	47,0	30,20		NW	1	Fine.
	2	0	54,0	49,5	30,22		NNE	1	Fine.
6	7	0	38,5	47,0	30,23		SSE	1	Fine.
	2	0	54,5	51,0	30,23		NE	1	Fair.
7	7	15	46,0	50,0	30,27		NNE	1	Fair.
	2	0	52,0	51,5	30,31		NE	2	Fair.
8	7	0	45,0	49,0	30,36		NNE	2	Fair.
	2	0	57,5	50,0	30,36		NNE	2	Fair.
9	7	0	42,5	47,5	30,36		N	1	Cloudy.
	2	0	50,5	49,5	30,355		N	1	Fine.
10	7	0	43,0	47,0	30,28		N	1	Fair.
	2	0	50,0	49,5	30,24		N by E	1	Fair.
11	7	0	45,5	48,0	30,16		SWbyW	1	Cloudy.
	2	0	57,5	52,0	30,13		W	1	Fair.
12	12	0	45,0	49,0	30,20		NE	1	Cloudy.
	2	0	54,5	51,5	30,22		NE by E	1	Fair.
13	7	0	46,0	50,0	30,26		S	1	Fine.
	2	0	56,0	52,5	30,26		NE	1	Fine.
14	7	0	43,0	49,5	30,21		S	1	Fine.
	2	0	62,0	54,0	30,07		S	2	Fine.
15	7	0	50,0	52,0	29,68		S by W	2	Cloudy.
	2	0	52,5	53,5	29,69		W by S	1	Fair.
16	7	15	47,0	50,5	29,50	0,407	SW	1	Rain.
	2	0	57,5	54,0	29,55		N by W	1	Fair.

METE-

METEOROLOGICAL JOURNAL
for 1775.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	50,5	53,0	29,625	0,025	WNW	1	Fine.
	2	0	59,0	55,5	29,625		N by W	1	Fair.
18	7	0	47,0	53,0	29,585		W by S	1	Fine.
	2	0	56,5	54,5	29,59		W by S	1	Fine.
19	7	0	41,0	50,0	29,63	0,118	W by N	1	Fine.
	2	0	53,5	52,5	29,725		W by N	1	Fine.
20	7	0	40,0	48,5	29,98		SSW	1	Fine.
	2	0	59,0	51,5	30,02		S	1	Fine.
21	7	0	47,0	49,0	29,99		E	1	Fine.
	2	0	60,0	53,5	29,90		ESE	1	Fine.
22	7	0	50,0	52,0	29,61	0,016	S	0	Rain.
	2	0	59,0	55,5	29,59		SW	1	Fair.
23	7	0	45,5	53,0	29,71		SW	1	Fine.
	2	0	54,0	54,5	29,85		SW	1	Fair.
24	7	0	48,0	53,5	30,02	0,116	SSW	1	Rain.
	2	0	62,0	57,0	30,08		NW	1	Fair.
25	7	0	50,0	55,0	30,19		SW	1	Fair.
	2	0	60,0	57,5	30,20		SSW	1	Fair.
26	7	0	53,5	56,5	30,18		S	1	Fair.
	2	0	72,0	61,0	30,15		S	1	Fine.
27	7	0	52,0	55,5	30,09		ENE	1	Fine.
	2	0	73,0	63,5	30,05		ESE	1	Fine.
28	7	0	55,0	57,5	30,06		NE	1	Fine.
	2	0	79,0	65,0	30,06		SW	1	Fine.
29	7	0	62,0	62,5	30,02		SE	1	Fine.
	2	0	83,5	69,0	30,02		S by E	1	Fine.
30	7	0	62,0	65,5	30,00		SE	1	Fine.
	2	0	74,0	69,0	30,01	0,386	W	1	Fine.

METEOROLOGICAL JOURNAL for 1775.

	Time. H. M.		Therm. without	Therm within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 1	7	0	54,5	63,5	30,12		NW	1	Cloudy.
	2	0	62,0	63,0	30,24		N	1	Fair.
2	7	0	51,5	60,5	30,39		SSW	1	Fair.
	2	0	68,5	63,5	30,40		W	1	Fine.
3	7	0	53,5	60,5	30,32		SSW	1	Fine.
	2	0	69,0	64,0	30,23		NW	1	Fine.
4	7	0	50,0	57,5	30,06		NE	1	Fair.
	2	0	73,0	65,5	29,95		S by E	1	Fine.
5	7	0	57,0	62,0	29,68		WSW	1	Fine.
	2	0	68,0	64,5	29,74		SSW	1	Rain.
6	7	0	49,5	57,0	29,96	0,232	N	2	Fine.
	2	0	60,5	59,0	30,06		NW	1	Fine.
7	7	0	56,0	57,5	30,14		SSW	1	Fine.
	2	0	67,0	61,0	30,13		SSW	1	Fair.
8	7	0	55,5	59,0	30,00	0,039	W	1	Cloudy.
	2	0	63,5	61,5	30,02		W by S	2	Cloudy.
9	7	0	56,0	59,0	30,08		W by S	1	Cloudy.
	2	0	67,0	63,0	30,08		SSW	2	Fine.
10	7	0	52,5	60,0	30,05		N	2	Fine.
	2	0	61,0	60,5	30,09		NW	2	Fine.
11	11	0	49,0	53,0	30,19		N	1	Fine.
	2	0	57,0	57,0	30,18		N.	1	Fine.
12	7	0	48,5	55,0	30,05		W	0	Rain.
	2	0	51,5	56,5	30,02		W	1	Rain.
13	7	0	49,0	54,5	30,12	0,058	NNE	1	Fair.
	2	0	59,5	56,0	30,15		E by N	1	Fine.
14	7	0	48,0	54,5	30,26		NNE	1	Fine.
	2	0	64,5	57,0	30,26		NW	1	Fine.
15	7	0	48,5	58,5	30,17		W by S	1	Fair.
	2	0	66,0	61,5	30,11		N by W	1	Fine.
16	7	0	52,0	57,5	30,02		WNW	1	Fine.
	2	0	68,5	61,0	29,86		W by S	2	Fine.

METEOROLOGICAL JOURNAL
for 1775.

	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 17	7	0	51,5	59,5	29,81		NW	1	Fine.
	2	0	61,0	60,5	29,81		NW	1	Fine.
18	7	0	47,0	54,0	30,06		N	1	Fair.
	2	0	55,0	56,0	30,12		NNE	1	Fine.
19	7	0	46,0	53,0	30,00		WSW	1	Rain.
	2	0	55,0	55,0	29,92		N	1	Rain.
20	7	0	43,0	50,0	30,03	0,171	N	1	Fine.
	2	0	52,5	52,5	30,12		N	1	Fair.
21	7	20	48,5	51,5	30,20		WSW	1	Rain.
	2	0	62,5	55,0	30,21		W by N	1	Fair.
22	7	0	51,5	55,0	30,24		NE	1	Fine.
	2	0	58,0	57,5	30,25		ENE	1	Fair.
23	7	0	50,0	55,0	30,18		NE	1	Cloudy.
	2	0	56,5	66,5	30,11		ENE	1	Fair.
24	7	0	48,0	54,5	29,99		ENE	1	Cloudy.
	2	0	65,0	58,0	29,885		SE	1	Fine.
25	7	0	52,0	57,0	29,83		NNW	1	Fair.
	2	0	55,0	58,0	29,88		N	1	Fair.
26	7	0	51,0	56,0	30,06		N	1	Fair.
	2	0	50,5	57,0	30,14		NE by N	1	Rain.
27	7	0	49,0	55,0	30,24	0,052	NW	1	Fair.
	2	0	64,0	58,0	30,27		NE by N	1	Fine.
28	7	0	55,5	57,5	30,34		W	1	Fair.
	2	0	67,0	60,5	30,37		N	1	Fair.
29	7	0	57,0	60,0	30,43		SW	1	Fine.
	2	0	68,5	63,5	30,41		WNW	1	Fair.
30	7	0	58,5	62,5	30,38		SE	1	Fine.
	2	0	70,0	64,5	30,37		SE	1	Fine.
31	7	0	59,0	62,5	30,34		SSW	1	Fair.
	2	0	74,5	67,0	30,31		NNE	1	Fine.

METEOROLOGICAL JOURNAL for 1775.

	Time.		Therm without	Therm within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str	
June 1	7	0	57,0	63,0	30,30		ENE	1	Fine.
	2	0	69,0	66,0	30,29		E	1	Fine.
2	7	0	52,0	60,5	30,27		NNE	2	Fair.
	2	0	68,5	63,0	30,19		NE	2	Fine.
3	7	0	56,5	61,5	30,18		NE	1	Fine.
	2	0	71,0	64,0	30,15		NE	1	Fine.
4	7	0	57,5	61,5	30,15		NE	1	Fine.
	2	0	73,0	65,0	30,15		E by N	1	Fine.
5	7	0	62,0	63,5	30,12		E by N	1	Fine.
	2	0	76,0	67,5	30,065		ESE	2	Fine.
6	7	30	67,0	67,5	29,985		E by N	1	Fine.
	2	0	81,5	73,5	29,92		E by N	1	Fine.
7	7	0	66,0	69,5	29,79	0,010	NNE	1	Fair.
	2	0	79,0	73,0	29,76		E by S	1	Fair.
8	7	0	66,5	71,0	29,78	0,010	ENE	1	Fine.
	2	0	73,5	73,0	29,86		S	1	Fine.
9	7	0	62,0	68,0	29,98		E by N	1	Fine.
	2	0	77,0	72,0	29,92		SE	1	Fine.
10	7	0	65,5	70,0	29,71		SSE	1	Cloudy.
	2	0	72,5	72,0	29,82		SE	1	Cloudy.
11	7	0	56,0	67,0	29,85	0,078	NNE	1	Rain.
	2	0	71,0	69,5	29 85		ENE	1	Fair.
12	7	0	61,5	67,0	29,845	0,122	SSE	0	Fair.
	2	0	74,0	70,0	29,91		SW	1	Fine.
13	7	0	62,0	67,0	29,99		ESE	1	Fine.
	2	0	78,0	71,0	30,00		S	0	Fine.
14	7	0	64,0	68,0	30,00		NE by E	0	Fine.
	2	0	78,0	71,5	29,97		E by N	1	Fine.
15	7	0	59,0	67,5	30,04		NE	1	Fair.
	2	0	73,5	71,0	30,055		NW	1	Fine.
16	7	0	59,5	67,5	30,055		N	1	Fine.
	2	0	73,5	69,0	29,99		N	1	Fine.

METEOROLOGICAL JOURNAL
for 1775.

	Time.		Therm. without	Therm. within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 17	7	0	60,0	65,5	29,855		ENE	1	Fine.
	2	0	78,0	70 0	29,76		SSW	0	Fine.
18	7	0	58,0	66,0	29,78		NE	1	Fair.
	2	0	76,0	69,5	29,83		NNE	1	Fair.
19	7	0	59,0	65,5	30,02		NE	1	Fair.
	2	0	67,5	68,5	30,11		NNE	1	Fair.
20	7	0	62,0	65,0	30,04		SW	1	Fair.
	2	0	77,0	70,5	29,99		WSW	1	Fair.
21	7	0	61,0	66,0	30,11		NW	1	Fine.
	2	0	77 0	69,5	30,11		W	1	Fair.
22	7	0	65,0	69,0	30,065		SW	1	Fair.
	2	0	78,0	73,0	29,93		WSW	2	Fine.
23	7	0	65,5	69,5	29,93		WSW	2	Fair.
	2	0	72,0	72,0	29,86		WSW	2	Fair.
24	7	0	60,5	68,0	29,77		W	1	Cloudy.
	2	0	62,0	68,5	29,72		SW	1	Rain.
25	7	0	56,5	65,0	29,74	0,028	SW	1	Fine.
	2	0	67,0	66,0	29,82		WSW	1	Fair.
26	7	0	58,0	64,0	29,91		NNE	1	Fair.
	2	0	69,5	66,5	29,89		NNE	1	Fair.
27	7	0	57,0	63,5	29,76		NNE	1	Fair.
	2	0	66,0	64,5	29,69		NE	1	Fair.
28	7	0	57,0	61,5	29,62		E by N	1	Fair.
	2	0	59,5	62,5	29,61		SSE	1	Rain.
29	7	0	58,0	62,5	29,70	0,373	SE	1	Fine.
	2	0	63,0	64,0	29,70		SWbyW	1	Rain.
30	7	0	59,0	64,5	29,80	0,637	W	1	Fair.
	2	0	67,0	65,5	29,855		SW	1	Fine.

METEOROLOGICAL JOURNAL

for 1775.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	59,5	64,5	29,92	0,263	WNW	1	Fine.
	2	0	71,0	66,5	29,94		W	1	Fine.
2	7	0	60,0	64,5	29,90	0,053	SW	1	Fair.
	2	0	67,5	66,5	29,85		S	1	Rain.
3	7	0	61,0	64,5	29,70	0,386	SW	1	Cloudy.
	2	0	73,0	67,5	29,72		SW by W	1	Fair.
4	7	0	61,0	66,5	29,98	0,024	W	1	Fair.
	2	0	69,5	67,0	30,03		W	1	Fair.
5	7	0	64,0	66,5	30,05		SW	1	Fair.
	2	0	73,0	69,0	30,04		SW by W	1	Fair.
6	7	0	62,5	67,5	29,72	0,091	NE	1	Rain.
	2	0	68,0	70,5	29,59		N	1	Rain, and thunder.
7	7	0	58,0	67,5	29,69	0,359	NNW	1	Rain.
	2	0	64,0	67,5	29,78		NW	1	Fair.
8	7	0	58,0	65,5	29,84	0,058	SW	1	Cloudy.
	2	0	70,5	68,5	29,85		SW	1	Fair.
9	7	0	61,5	65,5	29,70	0,061	S	1	Rain.
	2	0	72,0	68,0	29,74		SW by S	2	Fair.
10	7	0	64,5	67,5	29,84	0,016	WSW	1	Fair.
	2	0	70,5	68,5	29,84		SW by S	2	Fair.
11	7	0	62,0	66,0	29,87	0,010	SW	1	Fine.
	2	0	68,0	68,0	29,85		SSW	2	Fair.
12	7	0	63,5	66,5	29,85	0,020	SW	1	Fair.
	2	0	59,0	66,0	29,86		SSW	1	Rain.
13	7	0	60,5	65,0	29,89	0,677	SW	1	Fair.
	2	0	68,0	67,5	29,89		SW	2	Fair.
14	7	0	62,0	66,0	29,88	0,077	SSW	1	Fair.
	2	0	64,0	67,5	29,82		NW	1	Rain.
15	7	0	59,5	65,0	29,81	0,145	NW	1	Fair.
	2	0	66,0	66,5	29,88		NW	1	Fair.
16	7	0	59,0	64,0	29,92	0,024	NW	1	Fair.
	2	0	68,5	66,0	29,92		WNW	1	Fair.

METEOROLOGICAL JOURNAL
for 1775.

	Time.	Therm. without	Therm. with n.	Barom.	Rain	Winds.		Weather.
	H. M			inches.	Inch.	Points.	Str.	
July 17	7 0	59,0	63,5	30,00		NW	1	Fair.
	2 0	66,5	65,0	30,05		NW	1	Fair.
18	7 0	58,5	61,5	30,16		NNE	1	Fine.
	2 0	73,5	66,5	30,18		NW	1	Fine.
19	7 0	60,0	63,5	30,16		S W	1	Fine.
	2 0	76,0	68,0	30,14		W by S	1	Fine.
20	7 0	62,0	65,0	30,08		SW	1	Fine.
	2 0	79,5	70,5	30,05		SWbyW	1	Fine.
21	7 0	69,0	68,5	29,93		SSE	1	Fine.
	2 0	82,0	74,0	29,91		SSE	1	Fine.
22	7 0	66,0	70,0	29,85		NW	0	Hazy.
	2 0	77,0	73,0	29,84		NW	1	Fine.
23	7 0	63,0	70,0	29,82	0,123	NE	1	Cloudy.
	2 0	75,0	73,0	29,80		WSW	1	Fair
24	7 0	61,0	67,5	29 77	0,310	ENE	0	Fair.
	2 0	74,0	70,5	29,81		SW	1	Rain.
25	7 0	59,5	67,5	29 92	0,069	WSW	1	Cloudy.
	2 0	76,5	70,0	29,91		SW	1	Fair.
26	7 0	67,0	68,5	29,87		S	1	Fair.
	2 0	76,5	70,5	29,64		SWbyW	1	Fair.
27	7 0	67,5	69,5	29,78	0,042	SE	1	Fair.
	2 0	75,5	71,0	29,78		SW	1	Fine.
28	7 0	66,0	68,0	29,78		SSE	1	Fair.
	2 0	72,5	69,5	29,72		S	1	Rain.
29	7 0	59,0	66,0	29,79	1,058	SW	1	Thunder at 18h. fair.
	2 0	66,0	66,5	29,74		S	1	Fair, much thunder aftern.
30	7 0	60,0	63,5	29,81	0,498	W	1	Fair.
	2 0	69,0	67,0	29,95		WNW	2	Fair.
31	7 0	60,0	63,5	30,04		N	1	Fine.
	2 0	69,5	67,0	30,04		NNW	1	Fine.

METE-

METEOROLOGICAL JOURNAL

for 1775.

	Time. H. M.		Therm. without	Therm within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 1	7	0	61,0	63,5	29,81	0,038	E	1	Rain.
	2	0	72,5	68,0	29,77		WSW	1	Fair.
2	7	0	61,0	64,0	29,73	0,152	SSW	1	Rain.
	2	0	72,5	68,5	29,69		SSW	2	Fair.
3	7	0	62,5	65,0	29,705	0,154	SSW	1	Fine.
	2	0	72,0	68,0	29,73		SSW	1	Fair.
4	7	0	62,0	65,5	29,715	0,218	S	1	Fine.
	2	0	71,0	68,5	29,74		SSW	2	Fine.
5	7	0	61,0	65,5	29,83	0,032	SW	1	Fair.
	2	0	73,5	68,0	29,85		SSW	1	Fine.
6	7	0	60,0	64,5	29,95		NNE	1	Fine.
	2	0	73,0	69,0	29,96		NNW	1	Fine.
7	7	0	62,0	66,5	30,01		W	1	Fair.
	2	0	74,5	69,5	30,01		NW	1	Fine.
8	7	0	61,0	65,5	30,02		W	1	Fine.
	2	0	75,5	70,5	29,99		NW	1	Fine.
9	7	0	64,0	67,0	29,82		SSW	1	Cloudy.
	2	0	66,0	67,5	29,70		SSE	1	Rain.
10	7	0	58,0	63,5	29,88	0,220	WSW	2	Fine.
	2	0	70,5	65,5	29,95		NW	1	Fine.
11	7	30	59,0	63,0	29,88	0,010	S	1	Rain.
	2	0	66,0	66,0	29,80		SSW	2	Cloudy.
12	7	0	57,0	63,0	29,89	0,048	SSW	1	Fine.
	2	0	68,0	66,0	29,82		SSW	1	Cloudy.
13	7	0	57,0	61,5	29,91	0,118	SSW	1	Fair.
	2	0	68,5	64,0	29,92		W	2	Fair.
14	7	0	54,0	60,0	30,04	0,012	WSW	1	Fine.
	2	0	69,5	65,0	30,07		SW	1	Fine.
15	7	0	54,5	60,0	30,05		SE	1	Fog.
	2	0	69,5	64,5	30,00		SE	1	Fine.
16	7	0	56,0	58,0	29,93	0,043	SSE	1	Fair.
	2	30	68,0	63,5	29,98		N	1	Fine.

METE-

METEOROLOGICAL JOURNAL
for 1775.

	Time.		Therm. without	Therm. within.	Barom.	Rain	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 17	7	0	59,5	62,5	30,02	0,029	S	1	Rain.
	2	0	70,0	65,0	30,02		S by W	2	Cloudy.
18	7	0	66,0	66,0	30,05	0,023	SSW	1	Fair.
	2	0	72,5	69.5	30,06		SSW	1	Fair.
19	7	0	62,0	66,0	29,88		NNW	1	Fair.
	2	0	70,0	68,5	29,80		NW	1	Fair.
20	7	0	57,0	63,5	29,87		SSW	1	Fine.
	2	0	65,5	65,5	29,80		SSE	2	Fair.
21	7	0	52,5	61,0	29,88		SW	0	Fine.
	2	0	65,5	64,0	29,81		S	1	Rain.
22	7	0	59,5	63,0	29,50	0,171	SWbyW	2	Fair.
	2	0	69,0	65,5	29,73		W	2	Fine.
23	7	0	56,0	62,5	30,05		SSW	1	Fine.
	2	0	73,0	65,5	30,07		S	1	Fine.
24	7	0	57,0	62,5	30,07		SSW	1	Fine.
	2	0	72,0	66,0	30,04		NW	1	Fine.
25	7	0	55,0	63,0	30,00		NE	1	Fair.
	2	0	74,0	66,0	29,92		E by N	1	Fine.
26	7	0	58,0	64,0	29,74		SE	1	Fair.
	2	0	72,0	67,0	29,66		SSW	1	Fair.
27	7	0	52,5	62,5	29,75	0,358	WSW	1	Cloudy.
	2	0	66,5	65,5	29,76		WSW	2	Cloudy.
28	7	0	60,5	62,5	29,71	0,345	SW	1	Rain.
	2	0	62,5	65,5	29,75		SSW	[illegible]	Rain.
29	7	0	56,0	61,0	29,75	0,122	S	1	Fine.
	2	0	67,0	64,5	29,76		S by W	1	Fine.
30	7	0	56,0	61,0	29,77	0,042	SSW	1	Fine.
	2	0	69,0	64,5	29,79		SW	2	Fine.
31	7	0	58,0	62,0	29,54	0,068	SE by E	2	Rain.
	2	0	69,0	66,0	29,52		SSW	2	Fair.

METEOROLOGICAL JOURNAL
for 1775.

	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	56,5	61,0	29,85	0,171	SW	1	Fine.
	2	0	68,0	64,5	29,89		SSW	1	Fine.
2	7	0	63,0	63,5	29,85	0,115	SWbyW	2	Cloudy, much wind last night
	2	0	67,5	66,5	29,89		SWbyW	2	Cloudy.
3	7	0	56,5	63,5	30,00	0,020	WSW	1	Fair.
	2	0	56,0	63,5	30,05		S	1	Rain.
4	7	0	49,5	59,5	30,15	0,170	WSW	1	Fog.
	2	0	65,0	61,5	30,13		SE	1	Fine.
5	7	0	60,5	61,0	29,88	0,045	ENE	1	Fair.
	2	0	74,5	64,5	29,82		SE	1	Fine.
6	7	0	64,0	66,0	29,88		WSW	1	Fair.
	2	0	70,5	67,5	29,98		SW	1	Fine.
7	7	0	63,5	65,5	29,70	0,770	SSW	1	Fair, thunder last night.
	2	0	75,0	68,5	29,71		SW	1	Fine.
8	7	0	63,0	67,5	29,59	0,026	SSW	1	Fair.
	2	0	65,5	68,5	29,54		SW	1	Rain.
9	7	0	59,0	65,0	29,54	0,435	SSW	1	Fine.
	2	0	67,0	67,0	29,61		SSW	2	Cloudy.
10	7	0	57,5	63,5	29,61	0,283	SSE	1	Rain.
	2	0	68,5	66,5	29,61		SWbyW	1	Fair.
11	7	0	53,5	61,0	29,64	1,100	WSW	1	Fine.
	2	0	60,5	62,5	29,64		SSW	2	Rain.
12	7	0	47,0	56,0	29,90	0,097	WSW	1	Fine.
	2	0	60,5	59,0	29,91		W by S	1	Fine.
13	7	0	53,5	67,5	29,57	0,378	W	1	Fair.
	2	0	59,5	59,5	29,57		NW	1	Rain.
14	7	0	52,0	57,0	29,70	0,512	N	1	Rain.
	2	0	62,0	60,0	29,78		N by E	1	Fair.
15	7	0	53,5	56,5	29,68	0,184	NW	1	Fair.
	2	0	61,5	60,5	29,63		NNE	1	Cloudy.
16	7	0	54,0	57,5	29,59	0,497	NNE	1	Fair.
	2	0	62,0	59,5	29,71		WNW	1	Fine.

METEOROLOGICAL JOURNAL
for 1775.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	50,5	56,5	29,84		SW	1	Fair.
	2	0	65,0	60,0	29,83		SSW	1	Fine.
18	7	0	58,0	58,0	29,63		SSW	1	Cloudy.
	2	0	63,0	60,5	29,67		S by W	1	Cloudy.
19	7	0	62,0	61,5	29,70	0,282	S	1	Rain.
	2	0	66,0	63,5	29,79		SWbyW	1	Cloudy.
20	7	0	61,0	62,5	29,81	0,018	S	1	Fair.
	2	0	71,0	66,0	29,74		S	1	Fine.
21	7	0	61,0	63,5	29,51		S	1	Cloudy.
	2	0	66,5	65,0	29,60		S	2	Fine.
22	7	0	56,0	61,0	29,88	0,089	S	1	Fine.
	2	0	70,5	65,5	29,95		ESE	1	Fine.
23	7	0	56,5	60,5	29,90		ENE	1	Fine.
	2	0	69,5	66,0	29,87		ESE	1	Fine.
24	7	0	58,0	61,5	29,78		NE	1	Fine.
	2	0	71,5	67,0	29,75		ENE	1	Fine.
25	7	0	58,5	62,0	29,73		NE	1	Fair.
	2	0	70,5	66,5	29,72		E	1	Fine.
26	7	0	53,5	60,5	29,69		NNE	1	Fine.
	2	0	70,0	66,0	29,69		SSW	1	Fine.
27	7	0	57,5	62,5	29,79		WSW	1	Fair.
	2	0	64,0	64,0	29,85		W	1	Fair.
28	7	0	47,0	59,0	29,84		SW	1	Fine.
	2	0	63,0	61,5	29,84		SSW	1	Fair.
29	7	0	50,5	59,5	29,84		WSW	1	Fair.
	2	0	63,5	62,0	29,855		W	1	Fair.
30	7	0	50,0	58,5	29,82		SW	0	Fair.
	2	20	63,0	61,0	29,85		N by E	1	Fine.

METEOROLOGICAL JOURNAL for 1775.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	50,0	57,5	29,92		NE	1	Fair.
	2	0	64,5	61,0	29,94		NNE	1	Fine.
2	7	0	49,5	54,5	29,80		NE	0	Fog.
	2	0	63,5	59,0	29,68		NE by E	1	Fair.
3	7	0	56,0	58,5	29,40	0,037	N	1	Fair.
	2	0	63,0	60,0	29,42		WSW	1	Fine.
4	7	0	57,5	57,5	29,555		W	1	Cloudy.
	2	0	58,0	58,5	29,68		W	1	Fine.
5	7	0	40,5	52,5	29,82		SSW	1	Fine.
	2	0	57,0	54,0	29,81		W by N	1	Fine.
6	7	0	49,0	51,5	29,73		SSW	1	Cloudy.
	2	0	55,5	54,0	29,70		WSW	1	Rain.
7	7	0	51,0	52,5	29,63	0,257	S by W	1	Rain.
	2	0	53,0	54,0	29,76		W	1	Fine.
8	7	0	42,5	48,5	30,07	0,178	SW	1	Fine.
	2	0	57,5	52,5	30,09		WSW	1	Fine.
9	7	0	59,5	54,5	29,77	0,191	SW	1	Rain.
	2	0	66,0	57,5	29,84		SW	1	Fair.
10	7	0	47,5	55,5	30,00		SW	1	Fine.
	2	0	61,0	58,5	30,05		SW	1	Fair.
11	7	0	50,5	56,0	30,06		SW	1	Fair.
	2	0	62,5	68,0	30,07		WSW	1	Fine.
12	7	0	58,5	58,5	29,95		SSW	1	Cloudy.
	2	0	63,5	60,0	29,92		SW	1	Fair.
13	7	0	56,0	60,5	29,82	0,027	SW	1	Fair.
	2	0	63,0	61,5	29,84		WSW	1	Fair.
14	7	0	46,5	55,0	29,91		SSW	1	Fine.
	2	0	58,0	57,5	29,91		SW	1	Fine.
15	7	0	43,5	52,5	29,98		SW	1	Fair.
	2	0	57,0	54,5	30,05		W	1	Fair.
16	7	0	41,0	51,0	30,21		S	1	Fog.
	2	0	57,0	53,5	30,13		SWbyW	1	Fine.

METE-

METEOROLOGICAL JOURNAL for 1775.

	Time. H.	M.	Therm without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Oct 17	7	0	55,0	54,0	29,95	0,112	W	0	Fog.
	2	0	62,0	57,0	29,89		SW	0	Fair.
18	7	0	51,0	56,5	29,43	0,612	SW	1	Fair.
	2	0	55,0	57,5	29,48		W W	2	Fair.
19	7	0	41,5	53,0	29,70	0,208	SSW	1	Fine.
	2	0	53,0	53,5	29,42		S	2	Cloudy.
20	7	0	43,0	50,0	29,16	0,369	S by W	3	Windy night.
	2	0	49,0	50,5	29,23		WSW	2	Fine.
21	7	0	43,0	48,5	29,20		SW	1	Fine.
	2	0	54,5	50,5	29,55		WSW	2	Fine.
22	7	0	43,0	48,5	29,77	0,019	SW	1	Cloudy.
	2	0	44,5	49,0	29,69		ENE	1	Rain.
23	7	0	39,0	46,5	30,09	0,348	NW	1	Fine.
	2	0	51,0	48,5	30,20		NW	1	Fair.
24	7	0	42,5	47,5	30,05		S by W	1	Fair.
	2	0	47,0	49,5	29,98		NW	2	Rain.
25	7	0	37,0	45,0	30,04	0,010	N	1	Fine.
	2	0	44,0	45,5	30,16		NW	1	Fine.
26	7	0	32,0	41,5	30,25		W	1	Fine.
	2	0	46,0	43,0	30,26		NNW	1	Fine.
27	7	0	39,0	43,0	30,24		W	1	Fair.
	2	0	48,0	45,0	30,19		WSW	1	Fine.
28	7	0	44,0	45,0	29,98		SW	1	Cloudy.
	2	0	52,0	47,5	29,91		W	1	Fair.
29	7	0	40,0	46,5	30,00	0,020	N	1	Fine.
	3	30	48,0	47,0	30,07		NNW	1	Fair.
30	7	0	34,0	44,0	30,13		SSW	1	Fair.
	2	0	45,0	44,5	30,14		S	1	Fair.
31	7	0	41,0	44,0	29,94	0,039	E	0	Rain.
	2	0	47,0	45,5	29,80		SSW	1	Rain.

	Time. H.	M	Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Nov. 1	8	0	51,0	48,0	29,59	0,985	SSW	0	Rain.
	2	0	55,5	50,5	29,63		WNW	1	Fair.
2	8	0	41,0	46,0	29,75		NE	1	Fair.
	2	0	46,0	48,5	29,81		ENE	2	Fine.
3	8	0	40,0	44,5	29,84		ENE	1	Fair.
	2	0	46,5	46,0	29,84		ENE	1	Fair.
4	8	0	41,5	43,5	29,77		NE	1	Fair.
	2	0	46,0	45,0	29,72		ENE	1	Fair.
5	8	0	42,0	43,5	29,57	0,057	ENE	1	Rain.
	2	0	42,5	45,0	29,50		ENE	1	Rain.
6	8	0	46,0	46,0	29,42	0,133	ENE	1	Rain.
	2	0	48,0	47,5	29,37		ESE	1	Rain.
7	8	0	45,5	48,5	29,38	0,194	NE	1	Rain.
	2	0	47,0	49,0	29,33		NE	1	Rain.
8	8	0	46,0	48,5	29,41		N	1	Cloudy.
	2	0	48,5	49,5	29,49		NNW	1	Rain.
9	8	0	42,0	47,0	29,70		N	1	Fair.
	2	0	46,0	48,0	29,86		N by E	1	Fine.
10	8	0	33,0	44,5	30,00		SW	1	Fair.
	2	0	42,0	45,0	30,00		SW	1	Fair.
11	8	0	38,0	42,5	29,78		E	1	Fog.
	2	0	47,5	44,5	29,68		E	1	Fair.
12	8	0	45,5	45,5	29,49	0,063	ESE	1	Cloudy.
	2	0	57,5	47,5	29,37		SW	1	Rain.
13	8	0	53,5	51,5	29,01	0,119	S by W	2	Rain.
	2	0	55,0	53,0	29,00		WSW	1	Cloudy.
14	8	0	40,0	46,5	29,41	0,211	NW	2	Fine.
	2	0	44,5	46,5	29,57		NW by W	2	Fine.
15	8	0	33,5	42,0	29,69		SW	1	Fair.
	2	0	37,5	42,5	29,62		NW	1	Rain.
16	8	0	31,0	40,0	29,64	0,163	N	1	Fine.
	2	0	40,0	40,5	29,78		N	1	Fine.

METE-

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 17	8	0	37,5	40,0	29,68	0,019	SE	1	Rain.
	2	0	46,5	41,0	29,27		SSW	1	Rain.
18	8	0	41,0	44,0	29,16	0,758	NW	2	Cloudy.
	2	0	41,5	44,0	29,41		N	2	Cloudy.
19	8	0	35,0	40,5	30,05	0,048	N	1	Fine.
	2	0	39,0	41,0	30,14		N	1	Fine.
20	8	0	35,0	39,0	30,15		S by W	1	Fair.
	2	0	41,0	40,5	30,13		SSE	1	Cloudy.
21	8	0	40,0	41,0	30,30	0,072	SE	1	Cloudy.
	2	0	42,5	40,0	30,33		E	1	Fair.
22	8	0	28,0	37,0	30,40		N	1	Fine.
	2	0	36,5	38,0	30,36		NNE	1	Fair.
23	8	0	36,5	38,0	30,17	0,040	SW	1	Fog.
	2	0	40,5	39,0	30,15		NE	1	Cloudy.
24	8	0	36,0	38,0	30,15	0,025	NNE	1	Fair.
	2	0	41,0	39,5	30,12		NE	1	Fair.
25	8	0	36,0	40,0	30,06		NE	1	Fair.
	2	0	40,0	40,5	30,02		NNE	1	Cloudy.
26	8	0	35,5	40,0	30,01	0,025	SE	1	Cloudy.
	2	0	38,0	40,0	30,01		SSE	1	Rain.
27	8	0	37,5	40,0	29,99		E	1	Fog.
	2	0	39,5	40,5	29,94		ESE	1	Fair.
28	8	0	37,5	39,5	29,80		ENE	1	Rain.
	2	0	40,5	41,0	29,77		SE	1	Rain.
29	8	0	45,0	42,5	29,67	0,010	SSE	1	Fair.
	2	0	49,5	44,0	29,55		S	1	Rain.
30	8	0	54,5	48,0	29,89	0,019	SE	1	Fair.
	2	0	57,0	50,0	29,90		SSW	1	Fair.

METE-

METEOROLOGICAL JOURNAL

for 1775.

	Time.		Therm. without	Therm within	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	55 0	54,5	30,18		S	1	Fair.
	2	0	58,0	55,0	30,19		S	1	Fine.
2	8	0	51,0	54,0	30,20		SW	1	Fair.
	2	0	53,5	56,0	30,18		SW	1	Fine.
3	8	0	50,0	53,0	30,21		SW	1	Fair.
	2	0	55,5	54,5	30,22		WSW	1	Fair.
4	8	0	52,0	54,5	30,33		WSW	1	Fog.
	2	0	53,0	54,0	30,36		WSW	1	Cloudy.
5	8	0	47,5	53,5	30,41		WSW	1	Cloudy.
	2	0	49,5	53,0	30,40		SW	1	Cloudy.
6	8	0	42,0	48,0	30,46		N	1	Fine.
	2	0	47,0	50,0	30,48		N	1	Fine.
7	8	0	38,0	46,5	30,57		SSW	1	Fog.
	2	0	44,0	46,5	30,53		N by W	1	Fair.
8	8	0	45,0	46,5	30,45		WSW	1	Fog.
	2	0	49,0	47,5	30,40		SW	1	Cloudy.
9	8	0	37,0	44,0	30,45		N	1	Fine.
	2	0	41,5	44,0	30,49		N	1	Fine.
10	8	0	33,0	40,0	30,55		NNE	1	Fine.
	2	0	38,5	40,5	30,57		NNE	1	Fine.
11	8	0	32,5	37,5	30,51		SW	1	Fog.
	2	0	41,0	39,0	30,48		SSW	1	Fine.
12	8	0	34,5	38,0	30,45		SSW	1	Fine.
	2	0	40,0	40,0	30,42		S	1	Fine.
13	8	0	27,0	35,5	30,33		SW	1	Fine.
	2	0	40,0	37,0	30,26		SW	1	Fair.
14	8	0	46,5	40,0	30,20	0,030	W	0	Fog.
	2	0	48,0	43,0	30,24		N	1	Fair.
15	8	0	33,5	40,5	30,33		ENE	1	Fair.
	2	0	36,0	41,0	30,32		NNE	1	Fair.
16	8	0	34,5	38,0	30,40		NE	0	Foggy.
	2	0	41,0	39,0	30,41		NE	1	Foggy.

METEOROLOGICAL JOURNAL
for 1775.

	Time.		Therm. without	Therm. within.	Barom.	Rain	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	30,5	36,5	30,45		E	1	Fine.
	2	0	34,0	37,0	30,55		SE	1	Fine.
18	8	0	31,0	34,0	30,535		ENE	1	Fair.
	2	0	35,5	34,5	30,47		NE	1	Fine.
19	8	0	29,0	34,0	30,32		NE	1	Fair.
	2	0	37,5	35,0	30,29		ENE	1	Fog.
20	8	0	35,0	34,0	30,12		E	1	Fog.
	2	0	36,0	37,0	29,98		SE	1	Fair.
21	8	0	37,5	37,0	29,38		SSE	1	Fog.
	2	0	46,5	39,0	29,21		SSW	1	Fair.
22	8	0	41,0	42,5	29,30	0,050	SW	1	Fog.
	2	0	46,0	43,5	29,41		SW	1	Fine.
23	8	0	48,5	45,5	29,13	0,146	SSW	2	Rain.
	2	0	51,0	47,0	29,06		SSW	2	Fine.
24	8	0	45,0	46,0	28,80	0,114	SE by E	2	Rain.
	2	0	47,0	47,0	28,60		SSW	2	Rain.
25	8	0	38,5	44,0	28,97	0,035	SSE	1	Fine.
	3	0	45,5	47,0	28,99		SW	1	Fair.
26	8	0	40,0	43,0	29,42		WSW	0	Fair.
	2	0	43,5	43,5	29,50		SW	1	Fine.
27	8	0	33,0	41,5	29,80		SSW	1	Fog.
	2	0	40,0	42,0	29,86			1	Fair.
28	8	30	29,0	38,5	29,96		NNE	1	Foggy.
	2	0	38,0	39,0	29,86		SSE	1	Fair.
29	8	0	39,0	39,5	29,68	0,144	SSW	1	Rainy.
	2	0	43,5	41,5	29,81		NNW	1	Fair.
30	8	0	30,5	38,0	29,96		NNE	1	Foggy.
	2	0	40,0	39,0	29,91		S	1	Fine.
31	8	0	32,0	38,0	30,12	0,057	SSW	1	Fair.
	2	0	38,5	38,5	30,14		S	1	Fair.

METEOROLOGICAL JOURNAL
for January 1776.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	38,5	39,0	29,94		SE	1	Cloudy.
	2	0	41,0	40,0	29,82		S	1	Fine.
2	8	0	41,0	41,5	29,50	0,075	NNW	1	Rain.
	2	0	40,5	41,5	29,69		NNW	1	Cloudy.
3	8	0	36,0	39,5	30,09	0,065	NE	1	Cloudy.
	2	0	37,0	40,5	30,14		E	1	Fine.
4	8	0	39,0	38,5	29,77	0,031	SSE	1	Rain.
	2	0	44,0	40,0	29,50		S	2	Rain.
5	8	0	44,5	43,5	29,40	0,444	SW	1	Fair.
	2	0	48,5	45,5	29,48		WSW	1	Cloudy.
6	8	0	36,0	43,5	29,63		WSW	1	Fine.
	2	10	41,5	44,0	29,57		E by N	1	Fair.
7	8	30	33,0	39,5	29,245	0,153	ENE	2	Rain.
	2	0	31,0	38,0	29,35		ENE	2	Snow.
8	8	0	30,0	34,0	29,46	0,229	NE	1	Much snow last night.
	2	0	32,0	35,5	29,49		N	1	Fair.
9	8	0	30,0	34,5	29,63	0,079	N	1	Cloudy.
	2	0	28,5	33,5	29,67		ENE	1	Cloudy.
10	8	0	30,5	33,5	29,73		NNE	1	Cloudy.
	2	0	33,0	34,0	29,73		NW	1	Cloudy.
11	8	0	30,0	31,5	29,57		E	1	Cloudy.
	2	0	35,0	33,5	29,41		SE	1	Cloudy.
12	8	0	30,0	33,0	29,21	0,091	E	2	Much snow and wind last nt.
	2	0	32,0	34,0	29,21		ENE	2	Snow.
13	8	0	30,0	32,5	29,32		NE	2	Much snow and wind last nt.
	2	0	29,0	32,0	29,36		NE	2	Cloudy.
14	8	0	25,0	30,0	29,45		ENE	2	Snow.
	2	0	26,0	29,0	29,49		NE	2	Snow.
15	8	0	26,0	29,0	29,70		N	1	Cloudy.
	2	0	29,0	30,5	29,70		ENE	1	Snow.
16	8	0	25,5	29,0	29,71		NNE	1	Snow.
	2	0	30,5	30,5	29,70		NE	1	Snow.

METEOROLOGICAL JOURNAL
for January 1776.

	Time.	Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Jan 17	8 0	28,0	29,5	29,74		NNE	1	Cloudy.
	2 0	30,5	30,5	29,74		NNE	1	Cloudy.
18	8 0	33,0	32,0	29,87		ENE	1	Cloudy.
	2 0	30,0	31,5	29,90		ENE	1	Cloudy.
19	8 0	27,0	30,5	29,90		NE	1	Snow.
	2 0	27,0	31,0	29,85		NE	1	Fair.
20	8 0	23,5	29,0	29,70		N	1	Fair.
	2 0	24,0	29,0	29,70		N	1	Fine.
21	8 0	22,0	27,0	29,62		SSW	1	Cloudy.
	2 0	28,5	27,5	29,55		SSW	1	Fine.
22	8 0	30,5	28,5	29,41		E	1	Fair.
	2 0	·33,5	30,5	29,39		NE	1	Fine.
23	8 0	25,0	28,5	29,53		NE	1	Fair.
	2 0	33,0	30,0	29,58		NNE	1	Snow.
24	8 0	30,0	30,0	29,81		ENE	0	Cloudy.
	2 0	35,0	31,5	29,84		NE by E	1	Fair.
25	8 0	29,0	31,5	29,87		NE	1	Cloudy.
	2 0	33,0	32,5	29,85		SE	1	Fair.
26	8 0	26,0	30,5	29,82		E	1	Cloudy.
	2 0	26,0	30,5	29,82		SSE	2	Fine.
27	8 0	19,5	26,0	29,80		ENE	2	Snow.
	2 0	·20,5	25,5	29,79		ENE	2	Snow.
28	8 0	18,5	22,5	29,94		E	2	Snow.
	2 0	22,0	23,0	30,00		E	2	Fine.
29	8 0	14,5	20,0	29,91		SE	1	Fine.
	2 0	24,0	21,5	29,88		SE by E	1	Fair.
30	8 0	15,0	19,5	29,97		NE	1	Fair.
	2 0	21,0	20,5	30,02		E	1	Fine.
31	8 0	13,5	19,5	30,09		NE	1	Fair.
	2 0	23,5	20,5	30,07		E	1	Fine.

METEOROLOGICAL JOURNAL for February 1776.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	14,5	19,0	29,97		NE	1	Fair.
	2	0	31,5	31,5	29,86		E	1	Cloudy.
2	8	0	37,0	28,5	29,78		NE	0	Cloudy.
	2	0	40,5	31,0	29,57		SE by E	1	Fair.
3	8	0	37,0	34,5	29,34	0,093	SSE	1	Fog.
	2	0	44,0	36,0	29,51		S by W	1	Fine.
4	8	20	44,0	38,5	29,22		SW	2	Cloudy.
	2	0	48,0	41,0	29,22		SW	1	Cloudy.
5	8	0	40,5	42,0	29,16	0,153	WSW	2	Fine.
	2	0	48,0	44,0	29,11		SWbyW	2	Fine.
6	8	0	37,0	42,5	28,98	0,221	WSW	2	Fine.
	2	0	45,0	44,0	29,02		WSW	2	Fine.
7	8	0	43,0	42,0	28,91		SWbyW	2	Fair.
	2	0	47,5	43,5	29,07		SW	2	Fair.
8	8	0	40,0	42,5	29,66	0,190	SW	1	Rain.
	2	0	49,5	44,5	29,62		SSW	2	Cloudy.
9	8	0	41,6	44,5	29,78	0,190	SSW	1	Fine.
	2	0	45,5	45,5	29,58		S	2	Cloudy.
10	8	0	42,0	46,0	28,99	0,524	SWbyW	2	Rain.
	2	0	48,0	47,0	28,97		SW	2	Fair.
11	8	0	42,5	45,0	28,84	0,275	SW	2	Fair.
	2	0	45,5	46,0	28,84		SSW	2	Rain.
12	8	0	34,0	42,0	29,10		SSW	1	Fair.
	2	0	46,0	44,0	29,24		WSW	1	Fine.
13	8	0	36,5	42,5	29,68		SW	1	Fine.
	2	0	47,0	44,0	29,71		WSW	1	Fine.
14	8	0	47,0	45,0	29,51	0,163	SW	2	Fair.
	2	0	51,0	48,0	29,53		S by W	1	Fine.
15	8	0	38,0	44,5	29,96		SSW	1	Fine.
	2	0	48,0	46,5	29,91		SSW	2	Fair.
16	8	0	42,0	46,0	29,75	0,085	SW	1	Fine.
	2	0	49,5	47,5	29,78		SW	1	Fair.

METEOROLOGICAL JOURNAL
for February 1776.

	Time.		Therm. without	Therm. within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points	Str.	
Feb. 17	8	0	36,0	43,0	29,68	0,021	ENE	1	Rainy.
	2	0	43,5	42,5	29,53		E	1	Fair.
18	7	0	35,5	42,0	29,43	0,270	SSW	1	Fair.
	2	0	39,5	42,0	29,25		NE	1	Cloudy.
19	7	0	38,5	42,0	29,34	0,140	SW	1	Rainy.
	2	0	41,5	41,5	29,62		NW	1	Fair.
20	7	0	41,5	40,5	29,775	0,089	S	1	Rainy.
	2	0	50,0	44,0	29,80		W by S	1	Fine.
21	7	0	49,5	45,5	29,58		WSW	2	Cloudy.
	2	0	54,5	49,0	29,50		WSW	2	Fair.
22	7	0	36,5	44,0	29,62	0,412	SW	1	Fine.
	2	0	45,0	45,0	29,41		SSE	3	Rain.
23	7	0	35,0	40,5	29,38	0,148	WSW	1	Fine.
	2	0	45,5	44,0	29,50		WSW	2	Fair.
24	7	0	39,0	41,0	29,25	0,079	SSE	1	Rain.
	2	0	46,0	48,0	29,10		WSW	1	Rain.
25	7	0	37,0	40,0	29,50	0,102	N	1	Cloudy.
	2	0	43,0	41,5	29,55		SSW	1	Fair.
26	7	0	46,0	43,5	29,18		SSW	1	Cloudy.
	2	0	53,0	46,5	29,13		S by W	2	Cloudy.
27	7	0	39,5	43,0	29,155	0,149	SSW	2	Cloudy.
	2	0	48,0	45,0	29,26		WSW	2	Fair.
28	7	0	43,0	43,5	29,11	0,140	SSW	2	Fair.
	2	0	47,0	45,5	29,17		SW	2	Fair.
29	7	0	38,0	43,0	29,35	0,066	SSW	1	Fine.
	2	0	48,0	45,0	29,35		WSW	1	Fine.

METE-

	Thermometer without.			Thermometer within.			Barometer.		
1775.	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.
January,	54,0	25,5	42,7	52,0	30,5	43,5	30,38	29,30	29,84
February,	55,0	35,0	44,5	52,5	41,5	45,7	30,48	28,89	29,24
March,	58,0	28,5	43,9	53,0	37,0	46,1	30,61	29,07	29,67
April,	83,5	36,5	52,8	69,0	40,0	52,7	30,36	29,50	30,026
May,	74,5	43,0	57,8	67,5	50,0	58,5	30,43	29,68	30,12
June,	81,5	52,0	66,3	73,5	60,5	67,3	30,30	29,61	29,91
July,	82,0	58,0	66,1	74,0	61,5	67,3	30,18	29,59	29,88
Auguſt,	75,5	52,5	64,3	70,5	58,0	64,8	30,07	29,50	29,86
September,	75,0	47,0	61,1	68,5	56,0	62,6	30,15	29,51	29,756
October,	66,0	32,0	50,5	68,0	41,5	52,4	30,26	29,16	29,86
November,	57,0	28,0	42,5	53,0	37,0	43,7	30,36	29,16	29,76
December,	58,0	27,0	41,2	56,0	34,0	43,0	30,57	28,60	30,06
1776. January,	44,5	13,5	29,3	43,5	20,5	31,8	30,14	29,21	29,687
February,	49,5	14,5	42,6	46,0	19,0	42,4	29,97	28,84	29,408
Whole year, beginning with March 1775.			51,5			52,7			29,833

VARIATION

VARIATION-NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Daily Mean.
June 18	21 34	21 48	21 45	21 36	21 41
19	21 31	21 53	21 55	21 38	21 46
20	21 35	21 53	21 50	21 41	21 45
21	21 39		21 50	21 41	21 43
22	21 39	21 50	21 50	21 40	21 45
23	21 37	21 47	21 50	21 41	21 44
24	21 44	21 45	21 52	21 47	21 47
25	21 40	21 45	21 46	21 40	21 43
26	21 40	21 47	21 47	21 30	21 41
27	21 30	21 46	21 46	21 43	21 41
28	21 37	21 47	21 47	21 40	21 43
29	21 38	21 39	21 45	21 38	21 40
30	21 37	21 43	21 47	21 40	21 44
July 1	21 34	21 44	21 47	21 38	21 41
2	21 37	21 47	21 48	21 36	21 42
3	21 42	21 45	21 48	21 40	21 44
4	21 37	21 47	21 45	21 42	21 43
Means,	21 37	21 47	21 48	21 39	
Mean of all 21 43.					

DIPPING-

DIPPING-NEEDLE.

		7 h. A.M.	12 h. M.	2 h. P.M.	11 h.	Mean.
		° ′	° ′	° ′	° ′	° ′
Divided face of instrument to the West,	June 19	72 30	72 33	72 28	72 32	72 32
	20	72 30	72 28	72 32	72 30	
	21	72 38		72 32	72 25	
	22	72 27	72 38	72 32	72 30	
	23	72 26	72 30	72 28	72 32	
	24	72 38	72 28	72 30	72 26	
	25	72 34	72 37	72 40	72 32	
East,	26	72 33	72 33	72 34	72 24	72 32
	27	72 28	72 33	72 32	72 33	
	28	72 35	72 35	72 34	72 32	
	29	72 32	72 30	72 44	72 33	
June 29, Poles changed, marked end points upwards.						
East,	June 29			72 44	72 33	72 40
	30	72 35	72 45	72 45	72 45	
	July 1	72 33	72 45	72 45	72 32	
West,	2	72 24	72 15	72 17	72 20	72 17
	3	72 14	72 13	72 12	72 15	
	4	72 17	72 18	72 18	72 18	

Mean of the four means, or the true dip, 72° 30′.

THE END OF PART I. OF VOL. LXVI.

PHILOSOPHICAL

TRANSACTIONS.

PART II.

PHILOSOPHICAL TRANSACTIONS,

OF

THE ROYAL SOCIETY

OF

LONDON.

VOL. LXVI. For the Year 1776.

PART II.

LONDON,

PRINTED BY W. BOWYER AND J. NICHOLS:
FOR LOCKYER DAVIS, IN HOLBOURN,
PRINTER TO THE ROYAL SOCIETY.

MDCCLXXVII.

CONTENTS

TO

VOL. LXVI. PART II.

PHI-

PHILOSOPHICAL

TRANSACTIONS.

PART II.

XVIII. *An abridged State of the Weather at* London *for One Year, commencing with the Month of March* 1775, *collected from the Meteorological Journal of the* Royal Society. *By* S. Horſley, *LL. D. Sec. R. S.*

R. June 27, 1776.

TABLE I.

An abridged view of the WINDS at LONDON, for one year, beginning with March 1775.

	N	S	E	W	NW	SE	NE	SW		Rain.		Quarterly Rain.	Half-yearly Rain.
Mar.	1	1½	0	3	6½	1	1	17	31	1,854			
Apr.	1½	3½	0½	1½	3½	3	6½	10	30	1,068		3,474	
May	6	0	0	2½	6½	2	6½	7½	31	0,552			11,298
June	1	1	0½	1½	1	4½	14	6½	30	1,389			
July	1	2½	0	2	7½	2	2	14	31	4,232		7,824	
Aug.	0½	3	0½	2	3½	3½	1½	16½	31	2,203			
Sept.	0½	3½	0½	1½	1½	2½	5	15	30	5,192			
Oct.	1½	1½	0½	4	3½	0	2½	17½	31	2,919		10,560	
Nov.	4	0½	2	0	3	5½	9½	5½	30	2,449			15,813
Dec.	2½	2½	1	0½	1	3	6	14	30½	0,576	Half day miſſed in the Journ.		
Jan.	2½	1	4½	0	1½	3½	15½	2½	31	1,167		5,253	
Feb.	0½	1	1	0	0½	2	2	22	29	3,510			
	22½	21½	11	18½	39½	32½	72	148		27,111			

It

It appears, that the winds from the S.W. were again the moſt frequent of any, and next to theſe the winds from the N.E. Of the winds from the four cardinal points, the North was the moſt frequent, and the Eaſt the moſt rare. The autumn was the wetteſt quarter, and the ſpring the drieſt. The rain of the three ſummer months was almoſt half as much again as that of the three winter months; but the rain of the winter half-year exceeded that of the ſummer half-year by about one-ſixth of the rain of the whole year. September gave the greateſt quantity of rain, and May the leaſt of any ſingle month in the whole year.

TABLE II.

Sub-divifion of the S.W.

	WSW	SW	SSW	
March	5	5	7	17
April	3	4	3	10
May	3½	0½	3½	7½
June	3	3	0½	6½
July	4	7	3	14
Auguft	3	2½	11	16½
September	6	3½	5½	15
October	5	7½	5	17½
November	0½	2½	2½	5½
December	3	6	5	14
January	1	0½	1	2½
February	8	6½	7½	22
	45	48½	54½	148

TABLE III.

Sub-divifion of the N.E.

	ENE	NE	NNE	
March	0	1	0	1
April	1	3	2½	6½
May	2	1½	3	6½
June	6	4	4	14
July	0½	1	0½	2
Auguft	0½	0½	0½	1½
September	1½	1	2½	5
October	1	1	0½	2½
November	4	3½	2	9½
December	1½	2	2½	6
January	6½	6½	2½	15½
February	0½	1½	0	2
	25	26½	20½	72

TABLE

TABLE IV.

Sub-divifion of the S.E.

	ESE	SE	SSE	
March	0	0½	0½	1
April	1	1	1	3
May	0	1½	0½	2
June	1½	1½	1½	4½
July	0	0½	1½	2
Auguft	0½	1½	1½	3½
September	1	1	0½	2½
October	0	0	0	0
November	1½	2½	1½	5½
December	0½	1	1½	3
January	0½	2	1	3½
February	0½	0	1½	2
	7	13	12½	32½

TABLE V.

Sub-divifion of the N.W.

	WNW	NW	NNW	
March	0½	3½	2½	6½
April	1½	1	1	3½
May	1½	4	1	6½
June	0	1	0	1
July	1½	5	1	7½
Auguft	0	2½	1	3½
September	0½	1	0	1½
October	0½	2	1	3½
November	1	1½	0½	3
December	0	0	1	1
January	0	0½	1	1½
February	0	0½	0	0½
	7	22½	10	39½

Of the winds between the S. and W. thofe from the S.S.W. were this year the moft frequent.

Here follows a general ftate of the winds, according to the degrees in which they prevailed refpectively, collected from the five preceding tables.

ESE	WNW	NNW	E	SSE	SE	W	NNE	S	NW	N	ENE	NE	WSW	SW	SSW	
7	7	10	11	12½	13	18½	20½	21½	22½	22½	25	26½	45	48½	54½	365½

Miffed in the journal,

366

TABLE VI.

Shewing the number of fair and frosty days in each half-month and in the whole year.

	Fair 1st half.	Fair Latter half.	Fair days in whole month.	Frosty days 1st half.	Frosty days Latter half.	Frosty days in whole months.
March	6	7	13			
April	15	9	24			
May	13	13	26			
June	12	11	23			
July		9	10			
	5	7	12			
September	1	11	12			
October	11	6	17		1	
November	8	5	13		1	2
	15	9	24		6	8
January	5	11	16	10	15	25
February	7		9			
Total fair days,			199	Total frosty days, 41		

There were eleven snowy days in this year, all in January, with the wind between the N. and E. The first snow fell on the 7th, and introduced the great frost, which set in in the day-time: for on the 7th, at 8½ in the morning, it rained with the thermometer at 33°, wind E.N.E.; but, at 2 in the afternoon of the same day, the rain was turned into snow, and the thermometer was sunk to 31°. There was a short remission of the frost on the 18th, the thermometer at 8 in the morning of that day being at 33°; but it was sunk again to 30° at 2 in the afternoon. On the 31st, at 8 in the morning, it was at 13,5, and only one degree higher the next morning, February 1st. The frost broke in the night between

between the 1ſt and 2d of February, the wind yet continuing N.E., from which quarter it had ſet almoſt all the time the froſt laſted. It changed to the S.E. on the 2d, and on the 3d got into the S.W. where it remained almoſt all the reſt of the month.

The following table ſhews the quantity of rain that fell with each wind in each month and in the whole year. It appears, that the S.W. gave more than two-thirds of the rain of the whole year, which ſeems not to have been altogether owing to the wet quality of that wind, but in great meaſure to the greater length of time it blew than any other. The numbers at the bottom of the table ſhew the proportional wetneſs of each wind upon the whole. They are made from the numbers in the laſt horizontal row but one of this table compared with the numbers in the laſt horizontal row of TAB. I. For the wetneſs of each wind is in proportion as the quantity of rain it gave in the whole year directly, and the number of days it blew inverſely. The former is ſhewn by the numbers in the laſt row but one of TAB. VII.; and the latter by thoſe in the laſt row of TAB. I. It appears, that the South wind was the drieſt of all, the S.W. the wetteſt, and the W. the next wetteſt.

TABLE

TABLE VII.

Shewing the quantities of rain which fell severally with each wind in every month and in the whole year.

	N	S	E.	W	NW	SE	NE	SW	
Mar.	0	0		0,132	0,114	0,074	0,095	1,439	1,854
Apr.	0	0,016	0	0,386	0,143	0	0	0,523	1,068
May	0			0,058	0	0	0,052	0,442	0,552
June	0	0		0,768	0	0,495	0,098	0,028	1,389
July	0	0		0,155	0,528	0	0,214	3,335	4,232
Aug.	0	0,039	0,038	0	0	0,043	0	2,083	2,203
Sept.	0	0,107	0	0,378	0,184	0	0,542	3,981	5,192
Oct.	0,037	0	0,039	0	0,378	0	0	2,465	2,919
Nov.	0,211	0	0		0,969	0,208	0,409	0,652	2,449
Dec.	0	0	0	0,030	0	0	0	0,546	0,576
Jan.	0,079	0	0,091		0,075	0,031	0,447	0,444	1,167
Feb.	0	0,089	0		0	0,093	0,291	3,037	3,510
	0,327	0,251	0,168	1,907	2,391	0,944	2,148	18,975	27,111
	11+	9+	1,2—	82+	48	22—	23—	100	

ABLE

For Tria f the MOON's fluence

	New.	1ſt Qr.	Full.	Laſt Qr.				Corrected by exception.
	D. H.	D. H.	D. H.	D. H.				
Mar.	1 10	8 16	16 20	24 2 New. 30 21	⊙ ~2 ~4 ⊙8 ~10 ⊙13 ~19 ⊙20 ~26 ⊙31	9	5	6
	1ſt Qr.	Full.	Laſt Qr.	New.				
Apr.	7 11½	15 10	22 8	29 8	~16 ⊙17 ⊙~19 ⊙~22 ⊙~24 ~30	6	4	0
May	7 6½	14 20½	21 14	28 21	⊙1 ⊙~6 ⊙~8 ⊙~13 ⊙~20 ⊙~27	6	1	2
June	6 0	13 5	19 20	27 10	~7 ⊙8 ~11 ⊙12 ⊙~25 ~29	6	0	2
July	5 14	12 12	19 4	27 1	⊙4 ~6 ⊙16 ~23 ⊙27 ~28 ⊙30	7	4	7
Aug.	4 2	10 19	17 14	25 17	~1 ⊙5 ~9 ⊙14 ~16 ○18 ⊙~22 ~27	8	5	7
Sept.	3 12	9 2	16 4	24 0	⊙5 ~7 ⊙16 ~19 ⊙20 ⊙~22	6	2	2

Sept.	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		[illegible]	[illegible]	[illegible]	[illegible]
Oct.	1	20	8	13	15	22	24	$0\frac{1}{2}$	3 6 9 13 17 $\underline{20}$ 22 23 25 29 31			11	4	6	2
							First Qr. 31	4							
	Full.		Last Qr.		New.		First Qr.								
Nov.	7	$0\frac{1}{2}$	14	19	22	14	29	$11\frac{1}{2}$	1 5 7 12 14 15 $\underline{19}$ 21 23 24 26 28 30			13	7	6	3
Dec.	6	15	14	17	22	3	28	20	1 4 22 $\underline{25}$ 29 31			5	4	3	2
Jan.	5	7	13	13	20	$14\frac{1}{2}$	27	6	$\underline{2}$ 5 7			3	2	3	2
Feb.	4	2	12	6	19	1	25	19	2 3 5 6 $\underline{8}$ 11 14 $\underline{16}$ 20 $\underline{22}$ 25 27			12	7	8	5
												9[illegible]	[illegible]	31	27

In this table, the changes of weather, which, having not been reverfed within 24 hours, fell on the days of octagonal afpect, are diftinguifhed by a line drawn underneath the number.

From this table it appears, that, of 92 changes of weather in the whole year, 46 fell on the days of the Moon's pretended influence. And rejecting of these changes, all that were reversed within 24 hours, of 53 that remain in all, 27 fell on the days of lunar influence. And if from these again we reject the octantal days, confining the Moon's influence to the days of syzygie and quadrature, there still remain 14 of the 53 for these days.

Of the new Moons four only were attended with a change of weather, and of the full Moons three; namely, the new Moons of the months of March, July, December, and February; and the full Moons of October, November, January. Both the setting-in and the breaking of the great frost happened on days exempt from Lunar influence.

Upon the whole, the trial turns out more in favour of the Moon this year than it did the last. But still the changes were many more on the days confessedly exempt from her influence, than on those which have been supposed to be the most subject to it.

The greatest monthly height of the barometer was only four times in this year accompanied with a N.E. wind, namely, in the months of April, June, November, and February. It was five times attended with a S.W. namely, in March, May, August, September, and December; and the greatest height observed in the whole year was one of these, namely, in the month of March.

Once it was accompanied with the Eaſt wind, namely, in January; and twice with a N.W. namely, in July and October.

The leaſt monthly height was once accompanied with a N.E. namely, in January; ſix times with a S.W. namely, in April, May, Auguſt, October, December, and February; twice with the South wind, namely, in March and September; once with the North, namely, in July; once with a S.E. in June; and once with a N.W. in November.

I ſubjoin a general view of the winds and rain in the two months of January and February 1775, which are not included in the preceding tables.

1775	N	S	E	W	NW	SE	NE	SW	Rain.	
Jan.	0	0	2	2	10	3½	4	4½	1,724	5 days miſſed in the journal.
Feb.	0½	1½	0	1	4	1½	1½	17	1,928	1 day miſſed.
	0½	1½	2	3	14	5	5½	21½		

Comparing this with the table p. 168. of the laſt volume, I find the general ſtate of the winds and rain for the twelve months, beginning with March 1775, as follows:

N	S	E	W	NW	SE	NE	SW
23	21	16½	22½	51½	32½	72	119

R A I N.

Quarterly rain.		Half-yearly rain.		Year's rain.
Spring,	4,435	Summer,	12,486	24,662
Summer,	8,051			
Autumn,	6,718	Winter,	12,176	
Winter,	5,458			

But note, that in the space of thefe twelve months feven days in all were miffed in the journal.

XIX. *Extract of a Meteorological Journal for the Year* 1775, *kept at* Bristol, *by* Samuel Farr, *M. D.*

R. June 27, 1776.

Months.	Barometer.			
	Highest.	Lowest.	Mean.	Vicissitude.
January	30,13	29,08	29,70	81–2
February	30,29	28,74	29,72	113–4
March	30,38	28,75	29,69	73–2
April	30,22	29,42	29,67	41–2
May	30,20	29,63	29,93	55–2
June	30,09	29,40	29,72	40–2
July	29,96	29,50	29,68	26–$\frac{1}{2}$
August	29,89	29,30	29,61	30–1
September	29,93	29,30	29,53	33–1
October	30,13	28,90	29,70	42–$\frac{1}{2}$
November	30,22	28,90	29,50	95–2
December	30,36	28,49	29,85	46–1

An

An abridged Table of the WINDS for BRISTOL, for the Year 1775.

	N	E	W	S	NW	SE	NE	SW	Rain.	Frofty Days.	Thunder, &c.
Jan.	0	0	0	½	½	6	4½	18½	4,529	2	30. S.W.
Feb.	0	0	½	½	4½	4	1½	18	4,145	2	12. S.W.
Mar.	½	0	½	0	8	1	4	16	2,834	1	
Apr	0	0	1	½	8	4	6½	10	0,616	0	29 SE.
May	½	0	½	0	10	½	10	9½	0,332	0	
June	½	0	½	0	½	5½	12	11	4,288	0	{ 6. 9. 14. N.E. 11. 28 29. S.E.
July	0	0	0	½	5	2½	2	21	5,414	0	27. S W.
Aug	0	0	½	½	1	4	1	24	6,947	0	4, 5. S.W.
Sept.	0	0	0	2	2	7	3½	15½	2,936	4	{ Earthquake 8. S.W. Br 29,30 20. S.W.
Oct.	0	½	1	1	7	1½	5	15	3,358	1	19. with hail, S W.
Nov	0	½	0	1	3½	9½	10	5½	2,044	3	17 S.
Dec.	0	½	½	0	2	8	8	12	1,154	12	23. S.W.
	1½	1½	6	6½	53	53½	67	176	38,597	25	

WEATHER FOR THE YEAR 1775.

January. It froze hard the 1ſt; after that was wet till 19th; ſnowed on 20th; then rained to 24th, when it froze; ſnowed again 21ſt; was rainy to the 30th, when there was a thunder ſtorm at night; 31ſt fair.

February. Wet till the 14th; fair, with light ſhowers, to 23d, that rainy; the reſt of the month fair.

March. Cloudy to 4th; then rainy to 17th; dry to 5th; 26th ſtormy; 27th fair; on 28th ſnow; 29th fair; 30th ſnow and froſt at night.

April.

April. A little rain on 4th, 14th, 15th, 18th, 21ſt, and 30th; the reſt fair, and after 21ſt remarkably warm; lightned on 29th.

May. Fair to the 5th; rainy then and on 17th and 19th; the reſt quite fair.

June. Fair to the 5th; ſtormy, with frequent thunder, to 11th; fair to 14th; after this fair to 20th; wet to the 25th; 26th fair; 27th cloudy; a hard ſtorm on 28th and 29th; 30th fair.

July. Wet and ſtormy to 14th; after this fair to 24th; that and 25th ſtormy; 26th fair; a ſtorm on 27th; rain 28th, 29th; 30th and 31ſt fair.

Auguſt. Wet and ſtormy to 6th; that with 7th and 8th fair; 9th ſtormy; 10th fair; then ſtormy to 22d; fair to 26th; and then to the end ſtormy.

September. 1ſt and 2d wet; the 3d it froze and was dry to the 5th; after this ſtormy to 14th; and then to 20th; fair to 26th, when there was a ſtorm. The reſt of the month was dry, but cloudy.

October. Fair, with a ſlight ſtorm, on 4th to 8th; it rained then and on 9th; none till the 14th; wet to 22d; then fair to 31ſt, which was wet.

November. Dry to the 4th; after that wet; 8th fair; 9th froſty; 10th fair; wet to 19th; fair to ; froſt 19th and 24th.

December. Wet to the 5th; fair and after 8th froſty to 13th; rainy to 17th; then froſty again to 20th; after this wet to 25th; dry and froſty to 29th; then froſty on 30th, and wet on 31ſt.

XX. *Extract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1775. *By* Thomas Barker, *Esquire. Communicated by Sir* John Pringle, *Bart. P. R. S.*

R. June 27, 1776.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest	Lowest	Mean.	High.	Low.	Mean	High	Low.	Mean	
Jan.	Morn.	29,91	28,72	29,33	47	30	40½	50	20	36½	1,973
	Aftern.				48	31	41½	52½	26	41	
Feb.	Morn.	29,91	28,35	29,24	48	39½	44	49	31½	39	2,522
	Aftern.				49	41	45	51½	36	46	
Mar.	Morn.	30,09	28,61	29,32	48	38	44	46½	28	36½	1,728
	Aftern.				49½	39½	45	54	34	46½	
Apr.	Morn.	29,97	29,05	29,60	64½	40½	49	55	36	44	1,035
	Aftern.				67	42½	51	80	47	55½	
May	Morn.	29,94	29,31	29,67	62	49½	55½	58½	36	49	0,900
	Aftern.				64	50½	57	73	53	61	
June	Morn.	29,87	29,17	29,49	66½	58	62	62	50	56	0,887
	Aftern.				68	59	64	78	59	69½	
July	Morn.	29,71	29,16	29,41	66½	58½	63	63	52	58	4,078
	Aftern.				68	60	64½	78	58½	70	
Aug	Morn.	29,60	28,98	29,37	65	58½	62	61	48½	54½	4,760
	Aftern.				66	60	63	72	53	65	
Sept.	Morn.	29,67	29,02	29,31	64½	55½	60	60	45½	52½	5,670
	Aftern.				65½	56½	61	71	53	63	
Oct.	Morn.	29,80	28,50	29,38	59½	43½	51½	57½	30	43	3,480
	Aftern.				59½	45	52½	65	39	52	
Nov.	Morn.	29,96	28,50	29,34	48	39	42½	52	26½	36	3,570
	Aftern.				50	39	43	56	34	41½	
Dec.	Morn.	30,06	28,15	29,54	51	35½	42	52	24½	35	1,096
	Aftern.				51½	35½	42	55½	32	40	
											31,699

In

In four years, 1740, 41, 42, and 43, there came but in. 66,361 of rain. In the laft four years 1772, 73, 74, and 75, there was in. 124,957, which is nearly twice as much.

The proportion that the mean months bear to the whole years at feveral periods.

	1736–40	41–50	51–60	61–70	71–75	36–75
January	.054	.076	.078	.069	.070	.073
February	.051	.046	.052	.074	.073	.061
March	.047	.074	.066	.049	.058	.061
April	.057	.075	086	.056	035	.065
May	.075	.064	.073	.071	.094	.074
June	.075	.123	.097	.112	.079	.101
July	.139	.111	.134	.107	.072	.111
Auguft	.163	.059	.122	.099	.111	.105
September	.113	.095	.062	.074	.156	.092
October	.081	.094	.071	.115	.102	.093
November	.052	.105	.073	.100	.084	.086
December	.093	.078	.086	.074	.066	.078
	1.000	1.000	1.000	1.000	1.000	1.000

The year began favourable, the winter was mild and not in general wet; there was indeed a pretty deal of rain the firft half of February, but the latter part of that month was warm and forwarding, and the fpring continued to advance from that time with much fewer frofty mornings and N.E. winds than there frequently

are at that ſeaſon, the many ſtrong Weſterly winds keeping them back. The ſeed-time was fine, and the ſeaſon good for corn. There were Northerly winds the former part of April, but they were not ſharp ones; and the latter part of the month was hot, ſome days more ſo than in the height of ſummer.

The former part of the ſummer was fine, hot, and dry; ſome ſtony pariſhes burnt a good deal, eſpecially where the grounds were hard-ſtocked, and the crop of hay was but was ſmall; yet in general the graſs had got ſo forward in ſpring that it held out pretty well. There was a great deal of fine weather this year; and though there was a great deal of rain in the latter part of the ſummer, ſo much fine weather was intermixed with it that moſt of the hay and harveſt were got in well. Theſe rains began the beginning of July, were conſiderable but not frequent at firſt, came oftener toward the end of it and in Auguſt, and were almoſt continual the firſt three weeks of September, with ſeveral thunder-ſtorms. What harveſt was ſtill out, which in this country was chiefly peaſe and beans, was much ſpoiled; but in the fens and ſeveral other countries a good of barley was not finiſhed. The latter end of September and beginning of October were fine, and finiſhed the harveſt; but the rains returned again, and continued to the end of November, yet in leſs quantities than before, and the wheat ſeed-time was pretty good. The end of the year was fine and in general dry; at firſt warm, and afterward frequent froſty mornings, but no ſettled froſt. The dry weather

before Midſummer ſuited the wheat and barley, which were this year a good crop, and the grain large and fine, and cheaper than they have been for ſeveral years paſt.

The weather was leſs favourable in the South of England; the dry ſpring was drier and more burning; the barley of two growths, and ſome did not come up till Midſummer. The wet afterward was alſo greater, eſpecially in Hampſhire, ſo that their hay and harveſt ſuffered more than ours, and their barley, in particular, coming up late, was late ripe, and was half, or in ſome places moſt of it, damaged by the wet. The barley failed alſo in Norfolk, it not earing well on account of the dry ſeaſon.

For a good many years paſt, ſince the ſeaſons have been in general wet, the nature of Eaſt winds has been very different from what it was before. Several years after the great froſt in 1740 there were a great many N.E. winds in ſpring, but they were in general cold and dry, ſtopping vegetation; but for the laſt ten years, the Eaſt winds have been often very wet; many of the greateſt ſummer floods were by rain out of that quarter, and many times there came rain almoſt as certainly as the wind turned Eaſt.

An experiment of parting freſh-water from ſalt by freezing.

IN the ſevere froſt laſt January, ſome ſalt-water, being ſet abroad, froze into an ice which was not ſolid but porous, the hollows being filled with the ſalteſt part of the water, for the ice, when drained, was quite freſh.

 The

The ſalt-water, being again ſet abroad, froze as before; what remained ſtill unfrozen was now become exceeding ſalt, but the ice, drained and diſſolved, was little if at all brackiſh. This agrees with what Captain COOK mentions in his late voyage, that in 61° 35′ South latitude they filled their water caſks with freſh-water, melted out of ice found floating in the ſea. By this experiment, if another time more fully repeated, it may be found to what degree the ſaltneſs of water may be increaſed, by continuing to freeze away the freſh-water.

May not the knowledge of this be of uſe to the ſalt-makers, eſpecially in cold countries? The Sun is ſtrong enough of itſelf between the Tropics to dry away the ſea-water into ſalt; and, I think, at the ſalt-works near Lymington, they increaſe the ſaltneſs of the ſea-water by drying it away in the Sun before they boil it into ſalt. And this ſeems to be another means of parting freſh-water from the ſalt, which would ſave expence in boiling it away, and may be of uſe in the cold countries, and in winter.

XXI. *An Account of the Meteorological Instruments used at the* Royal Society's *House.* *By the Hon.* Henry Cavendish, *F. R. S.*

R. March 14, 1776.

Of the thermometers, with reflections concerning some precautions necessary to be used in making experiments with those instruments, and in adjusting their fixed points.

THE thermometers are both adjusted to FAHRENHEIT's scale: that without doors is placed out of a two-pair-of-stairs window, looking to the North, and stands about two or three inches from the wall, that it may be the more exposed to the air, and the less affected by the heat and cold of the house. The situation is tolerably airy, as neither the buildings opposite to it, nor those on each side, are elevated above it in an angle of more than 12°; but as the opposite building is only twenty-five feet distant, perhaps the heat may be a little increased at the time of the afternoon observation by the reflection from thence. In the middle of summer the Sun shines on the wall of the house, against which the thermometer is fixed, for an hour or two before the morning observation, but never

never ſhines on the thermometer itſelf, or that part of the wall cloſe to it, except in the afternoon, long after the time of obſerving. On the whole, the ſituation is not altogether ſuch as could be wiſhed, but is the beſt the houſe afforded.

The thermometer within doors is intended chiefly for correcting the heights of the barometer, and is therefore placed cloſe to it. The room in which it is kept looks to the North, and has ſometimes a fire in it, but not often.

It has been too common a cuſtom, both in making experiments with thermometers and in adjuſting their fixed points, to pay no regard to the heat of that part of the quickſilver which is contained in the tube, though this is a circumſtance which ought by no means to be diſregarded; for a thermometer, dipped into a liquor of the heat of boiling water, will ſtand at leaſt 2° higher, if it is immerſed to ſuch a depth that the quickſilver in the tube is heated to the ſame degree as that in the ball, than if it is immerſed no lower than the freezing point, and the reſt of the tube is not much warmer than the air. The only accurate method is, to take care that all parts of the quickſilver ſhould be heated equally. For this reaſon, in trying the heat of liquors much hotter or colder than the air, the thermometer ought, if poſſible, to be immerſed almoſt as far as to the top of the column of quickſilver in the tube. As this, however, would frequently be attended with great inconvenience, the obſerver will often be obliged to content himſelf with immerſing it to a much leſs depth; but then, as the quickſilver in a great part of the

the tube will be of a different heat from that in the ball, it will be neceffary to apply a correction on that account to the heat fhewn by the thermometer; to facilitate which the following table is given, in which the upper horizontal line is the length of the column of quickfilver contained in that part of the tube which is not immerfed in the liquor expreffed in degrees; the firft perpendicular column is the fuppofed difference of heat of the quickfilver in that part of the tube and in the ball; and the correfponding numbers in the table fhew how much higher or lower the thermometer ftands than it ought to do. The foundation on which the table is computed is, that quickfilver expands one 11500th part of its bulk by each degree of heat.

Diff. of Heat	Degrees not immerfed in the liquors.														
	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750
50	,2	,4	,7	,9	1,1	1,3	1,5	1,7	2	2,2	2,4	2,6	2,8	3,1	3,3
100	4,	,9	1,3	1,8	2,2	2,6	3,0	3,5	3,9	4,4	4,8	5,2	5,7	6,1	6,6
150	7,	1,3	2,0	2,6	3,3	3,8	4,6	5,2	5,9	6,5	7,2	7,9	8,4	9,2	9,8
200	9,	1,8	2,6	3,5	4,4	5,1	6,1	7,0	7,8	8,7	9,6	10	11	12	13
250	1,1	2,2	3,3	4,4	5,5	6,4	7,6	8,7	9,8	11	12	13	14	15	16
300	1,3,	2,6	3,8	5,1	6,4	7,7	9,1	10	12	13	14	16	17	18	20
350	1,5	3,0	4,6	6,1	7,6	9,1	11	12	14	15	17	18	20	21	23
400	1,7	3,5	5,2	7,0	8,7	10	12	14	16	17	19	21	23	24	26
450	2	3,9	5,9	7,8	9,8	12	14	16	18	20	22	24	25	27	29
500	2,2	4,4	6,5	8,7	11	13	15	17	20	22	24	26	28	31	33
550	2,4	4,8	7,2	9,6	12	14	17.	19	22	24	26	29	31	34	36

But as the generality of obfervers will be apt to neglect this correction, it would be proper to form two

two ſets of diviſions on ſuch thermometers as are intended for trying the heat of liquors; one of which ſhould be uſed when the tube is immerſed almoſt to the top of the column of quickſilver; and the other, when not much more than the ball is immerſed; in which laſt caſe the obſerver ſhould be careful, that the tube ſhould be as little heated by the ſteam of the liquor as poſſible. It muſt be obſerved, however, that the heat of the liquor may be eſtimated with much more accuracy by the firſt ſet of diviſions, with the help of the correction, than it can by the ſecond ſet, as the latter method is juſt only in one particular heat of the atmoſphere, namely, that to which the diviſions are adapted; but, if they are adapted to the mean heat of the climate for which the thermometer is intended, the error can never be very great, and, when the liquor is much hotter or colder than the air of that climate ever is, will be much leſs than if the firſt ſet of diviſions were uſed without any correction; but, when the liquor is within the limits of the heat of the atmoſphere, greater accuracy will ſometimes be obtained by uſing the firſt ſet of diviſions than the ſecond, for which reaſon the latter ſet ſhould not be continued within thoſe limits. I would willingly have given rules for the conſtruction of this ſecond ſet of diviſions, but am obliged to omit it, as it cannot be done properly without firſt determining, by experiment, how much the quickſilver in the tube is heated by immerſing the ball in hot liquors.

In a ſpirit thermometer, the error proceeding from the fluid in the tube being not of the ſame heat as that in the

the ball, is much greater; as ſpirits of wine expand much more by heat than quickſilver: for which reaſon ſpirit thermometers are not ſo proper for trying the heat of liquors as thoſe of quickſilver.

Another circumſtance which ought to be attended to in adjuſting the boiling point of a thermometer is, that the ball ſhould not be immerſed deep in the water; for, if it is, the fluid which ſurrounds it will be compreſſed by conſiderably more than the weight of the atmoſphere, and will therefore acquire a ſenſibly greater heat than it would otherwiſe do. The moſt convenient veſſel I know for adjuſting the boiling point is repreſented in fig. 1. ABCD is the veſſel; AB the cover, made to take on and off readily; E a chimney to carry off the ſteam; FG the thermometer, paſſed through a hole M*m* in the cover, and reſting in a little bag faſtened to the wire HK, intended to prevent the ball from being broken by accidentally falling to the bottom. This wire is made ſo as to be raiſed higher or lower at pleaſure, and muſt be placed at ſuch a height that the boiling point ſhall riſe very little above the cover. The hole M*m* is ſtopped with bits of cork or tow. By this means, as the tube is incloſed in a veſſel intirely filled with the ſteam of boiling water, the quickſilver in it is heated to the ſame degree as that in the ball; and beſides, that part of the tube, on which the boiling point is to be placed, is defended from the vapour, ſo that it is eaſy making a mark on the glaſs with ink. If ſuch a veſſel as this is uſed, the thermometer will be found to ſtand not ſenſibly higher

 when

when the water boils vehemently than when it boils gently; and if the mouth of the chimney is covered by any light body, in ſuch manner as to leave no more paſſage for the ſteam than what is neceſſary to prevent the body from being blown off by the preſſure of the included vapour, the thermometer will ſtand only half or three quarters of a degree higher, if the ball is immerſed a little way in the water, than if it is expoſed only to the ſteam. But if the covering of the chimney is removed, the thermometer will immediately ſink ſeveral degrees, when the ball is expoſed only to the ſteam, at leaſt if the cover does not fit cloſe; whereas when the ball is immerſed in the water, the removal of the covering has ſcarce any effect upon it. Whence it appears, that the ſteam of water boiling in a veſſel, from which the air is perfectly excluded, is a little but not much cooler than the water itſelf, but is conſiderably ſo if the air has the leaſt admiſſion to the veſſel. Perhaps a ſtill more convenient method of adjuſting the boiling point would be not to immerſe the ball in the water at all, but to expoſe it only to the ſteam, as thereby the trouble of keeping the water in the veſſel to the right depth would be avoided; and beſides, ſeveral thermometers might be adjuſted at the ſame time, which cannot be done with proper accuracy when they are immerſed in the water, unleſs the diſtance of the boiling point from the ball is nearly the ſame in all of them. At preſent there is ſo little uniformity obſerved in the manner of adjuſting thermometers, that the boiling point, in inſtruments made by our

beſt artiſts, differ from one another by not leſs than $2\frac{1}{2}°$; owing partly to a difference in the height of the barometer at which they were adjuſted, and partly to the quickſilver in the tube being more heated in the method uſed by ſome perſons than in that uſed by others. It is very much to be wiſhed, therefore, that ſome means were uſed to eſtabliſh an uniform method of proceeding; and there are none which ſeem more proper, or more likely to be effectual, than that the Royal Society ſhould take it into conſideration, and recommend that method of proceeding which ſhall appear to them to be moſt expedient.

Of the barometer, rain-gage, wind, and hygrometer.

THE barometer is of the ciſtern kind, and the height of the quickſilver is eſtimated by the top of its convex ſurface, and not by the edge where it touches the glaſs, the index being properly adapted for that purpoſe. This manner of obſerving appears to me more accurate than the other; becauſe if the quickſilver ſhould adhere leſs to the tube, or be leſs convex at one time than another, the edge will, in all probability, be more affected by this inequality than the ſurface. I prefer the ciſtern to the ſyphon barometer, becauſe both the trouble of obſerving and error of obſervation are leſs; as in the latter we are liable to an error in obſerving both legs. Moreover, the quickſilver can hardly fail of ſettling truer in the former

than in the latter; for the error in the ſettling of the quickſilver can proceed only from the adheſion of its edge to the ſides of the tube; now the latter is affected by the adheſion in two legs, and the former by that in only one: and, beſides, as the air has neceſſarily acceſs to the lower leg of the ſyphon barometer, the adheſion of the quickſilver in it to the tube will moſt likely be different, according to the degree of dryneſs or cleanneſs of the glaſs. It is true, as Mr. DE LUC obſerves, that the ciſtern barometer does not give the true preſſure of the atmoſphere; the quickſilver in it being a little depreſſed on the ſame principle as in capillary tubes. But this does not appear to me a ſufficient reaſon for rejecting the uſe of them. It is better, I think, where ſo much nicety is required, to determine, by experiment, how much the quickſilver is depreſſed in tubes of a given bore, and to allow accordingly.

By ſome experiments which have been made on this ſubject by my father Lord CHARLES CAVENDISH, the depreſſion appears to be as in the following table:

Inſide diameter of tube.	Grains of quickſilver in one inch of tube.	Depreſſ. of ſurface of quickſilver.	Inſide diameter.	Grains of quickſilver.	Depreſſ. of ſurface.	Inſide diameter.	Grains of quickſilver.	Depreſſ. of ſurface.
,6	972	,005	,35	331	,025	,20	108	,067
,5	675	,007	,30	243	,036	,15	61	,092
,4	432	,015	,25	169	,050	,10	27	1,40

The

The firſt column is the inſide diameter of the tube, expreſſed in decimals of an inch; the ſecond is the weight of a quantity of quickſilver ſufficient to fill one inch in length of it; and the third is the correſponding depreſſion of the convex ſurface of the quickſilver in a ciſtern barometer, whoſe tube is of that ſize. The reaſon of giving the ſecond column is, becauſe the eaſieſt way of aſcertaining the inſide diameter of the tube is, by finding the quantity of quickſilver ſufficient to fill a given length of it. It is needleſs ſaying, that the part of the tube, whoſe diameter is to be meaſured, is that anſwering to the upper part of the column of quickſilver; and that the table can be of no uſe but to thoſe only who obſerve by the convex ſurface.

In this barometer, the inſide diameter of the tube is about ,25 of an inch, and conſequently the depreſſion is ,05; the area of the ciſtern is near 120 times as great as that of the bore of the tube; ſo that as the quantity of quickſilver was adjuſted when the barometer ſtood at $29\frac{3}{4}$, the error ariſing from the alteration of the height of the quickſilver in the ciſtern can ſcarce ever amount to ſo much as $\frac{1}{100}$th of an inch. As the tube appeared to be well filled, it was thought unneceſſary to have the quickſilver boiled in it; but that is certainly the ſureſt way of filling a barometer well.

The principal reaſon of ſetting down the mean heat of the thermometer within doors, during each month, in the journal of the weather, is this: ſuppoſe that any one deſires to find the mean height of the barometer in any

any month, corrected on account of the heat of the quickſilver in the tube; that is, to find what would have been the mean height, if the quickſilver in the tube had been conſtantly of a certain given heat. To do this it is ſufficient to take the mean height of the barometer, and correct that according to the mean heat of the thermometer; the reſult will be exactly the ſame as if each obſervation had been corrected ſeparately, and a mean of the corrected obſervations taken. For example, ſuppoſe it is deſired to find what would have been the mean height of the barometer in the month of Auguſt 1775, if the quickſilver during that time had been always at 50 degrees of heat: the mean of the obſerved heights is 29,86 inches, and the mean heat of the thermometer is 65° or 50+15. The alteration of the height of the barometer by 15° of heat, according to M. DE LUC's rule, is ,047 inches; conſequently, the corrected mean height is 29,813.

The veſſel which receives the rain is a conical funnel, ſtrengthened at the top by a braſs ring, twelve inches in diameter. The ſides of the funnel and inner lip of the braſs ring are inclined to the horizon, in an angle of above 65°; and the outer lip in an angle of above 50° (a); which are ſuch degrees of ſteepneſs, that there ſeems no probability either that any rain which falls within the funnel, or on the inner lip of the ring, ſhould daſh out,

(a) To make what is here ſaid the more intelligible, there is, in fig. 2. given a vertical ſection of the funnel, ABC and *abc* being the braſs ring, BA and *ba* the inner lip, and BC and *bc* the outer.

or

or that any which falls on the outer lip ſhould daſh into the funnel. This veſſel is placed on ſome flat leads on the top of the Society's Houſe. It can hardly be ſcreened from any rain by the chimnies, as none of them are elevated above it in an angle of more than 25°; and as it is raiſed $3\frac{1}{2}$ feet above the roof, there ſeems no danger of any rain daſhing into it by rebounding from the lead.

The ſtrength of the wind is divided in the journal into three degrees; namely, gentle, briſk, and violent or ſtormy, which are diſtinguiſhed by the figures 1, 2, and 3. When there is no ſenſible wind it is diſtinguiſhed by a cypher.

In the future journals of the weather will be given obſervations of the hygrometer. The inſtrument intended to be uſed is of Mr. SMEATON's conſtruction, and is deſcribed in Phil. Tranſ. vol. LXI. p. 198. It is kept in a wooden caſe, made ſo as to exclude the rain, but to leave a free paſſage for the wind, and placed in the open air, where the Sun ſcarce ever ſhines on it. The inſtrument and caſe are both a preſent to the Society from Mr. SMEATON. The hygrometer was laſt adjuſted in Dec. 1775, and as the ſtring has now been in uſe upwards of five years, it is not likely to want re-adjuſting ſoon.

Of the Variation Compaſs.

IN this inſtrument, the box which holds the needle is not fixed, but turns horizontally on a center, and has an index faſtened to it, pointing to a divided arch on the braſs-

braſs frame on which it turns; and the method of obſerving is to move the box, till a line drawn on it points exactly to the end of the needle; which being done, the angle that the needle makes with the ſide of the frame is ſhewn by the index. Fig. 3. is the plan of the inſtrument; AB*ba* is the braſs frame, the ſides AB and *ab* being parallel; E*e* is a circular plate faſtened thereto, on which CD*dc*, the box which holds the needle, turns as on a center; N*n* is the needle, the pin on which it vibrates, being fixed in the center of the plate E*e*; B*b* is the diviſion on the braſs frame; and G the index faſtened to the box CD*dc*, furniſhed with a vernier diviſion; the diviſion and vernier being conſtructed ſo as to ſhew the angle which the line F*f* makes with AB or *ab*. The inſtrument is placed in the meridian by the teleſcope M*m*, the line of collimation of which is parallel to AB, and is pointed to a mark fixed due North of it.

Fig. 4. is a vertical ſection of the inſtrument paſſing along the line F*f*; AB is the braſs frame; CD*dc* the box which holds the needle; E*e* the circular plate on which it turns: N*n* is the needle; P and *p* are ſmall plates of braſs fixed to the ends of it, on each of which is drawn a line ſerving by way of index. Theſe pieces of braſs are raiſed to ſuch a height that their tops are on a level with the point of the pin on which the needle turns. The uſe of them is, that it is much eaſier obſerving this way than when the lines, ſerving by way of index, are drawn on the needle itſelf, as by this means the inconvenience proceeding from one kind of vibration in the needle is avoided.

avoided. S and *s* are two brass plates, on each of which is drawn a line to which the index at the end of the needle is to point; there is also a line parallel to these drawn on the bottom of the box; these three lines form the line F*f* in fig. 3. R is a double microscope intended to assist us in judging when the index P points exactly to the line F, that is, to the line drawn on the plate S. It is placed so, that a wire W*w* in its *focus* appears to coincide with this line; and in observing, the box is moved till the wire appears also to coincide with the index P.

The cap in the center of the needle is made to take on and off readily, and to fit on upon either face; so that we may on occasion observe with the under face of the needle uppermost, as is represented in fig. 5. But the regular observations are always made with the needle in its upright position, and by the help of the index P only; the intention of the other index and of inverting the needle is, to shew whether the line joining the indices P and *p*, or the line P*p* as I shall call it, is parallel to the direction of magnetism in the needle, and thereby to find whether, in the usual method of observing, the index G shews the true angle which the direction of magnetism makes with the side AB. The way of doing this is as follows; having suffered the needle to settle, the observer moves the box by means of the adjusting screw T, till the index P coincides with the line F, and reads off the angle shewn by the vernier. He then moves the box till the other index *p* coincides with the line *f*, which, as the pin on which the needle is suspended is fixed to the brass

 frame,

frame, may be done without any danger of altering the position of the needle or making it vibrate, and reads off the angle as before. The mean of these two is the true angle which the line P*p* makes with the side AB, supposing the division and vernier to be rightly constructed, even though neither the lines P*p* nor F*f* should pass through the center of the pin. Having done this, he takes off the cap and inverts the needle, and observes by both indices as before. It is plain, that if the line P*p* is parallel to the direction of magnetism in the needle, this mean will agree with the former, supposing that the magnetic variation has not altered between the observations. On the other hand, if it is not parallel to the direction of magnetism, but makes the variation appear greater than it ought to do when the needle is upright, it will make it appear as much less when the needle is inverted; so that the mean of the two abovementioned means is the true angle which the direction of magnetism in the needle makes with the side AB; that is, the true variation of the needle at that time and place, supposing AB to be placed accurately in the meridian. Having thus found the true angle which the direction of magnetism makes with AB, he subtracts that shewn by the index P in the upright position of the needle; the difference is the error of the instrument in the usual manner of observing.

It was by this method that the error of the instrument, at the time of the observations in 1774, was found to be 10′. For example, by a mean of the obser-vations

vations made on Sept. 5. the variation with the needle, in its upright position, was 21.36 by the South end, and 21.27 by the North; with the needle inverted it was 21.19 by the South end, and 21.29 by the North. The mean of all four is 21.28, which is the true variation at that time and place(b), and is 8′ less than that shewn in the upright position of the needle by the South end, which is the end always used in observing; so that by this day's experiment the error of the instrument appeared to be 8′; but by a mean of the observations of this and two other days it came out 10′. Since that time the needle has been altered; and, at the time of the observations in 1775, the error was so small as to be scarcely sensible.

Great care was taken that the metal, of which this variation compass is composed, should be perfectly free from magnetism. There is a contrivance in it for lifting the needle from off the point, and letting it down gently, to prevent injury in carrying from one room to another. The instrument is constructed nearly on the same plan as some made by the late Dr. KNIGHT. The principal difference is, that in his the pin which carried the needle was not fixed to the lower frame as in this, but to the box; the consequence of which was, that when the needle had settled, and the box was moved to make the index on the needle point to the proper mark, it was again put

(*b*) The quantity found by taking a mean of all the four numbers is evidently the same as that got by taking a mean of the two first and of the two last, and taking a mean of those two means.

into vibration, which caufed great trouble to the obferver. This inconvenience is intirely removed by the prefent conftruction. There is no other material difference, except that of the needle being made to invert, and the addition of the telefcope. The contrivance of fixing the pin which carries the needle to the lower frame, is taken from an inftrument of Lord CHARLES CAVENDISH; that of making the needle invert I have feen in fome compaffes made by SISSON.

There is a very common fault in the agate-caps ufually made for needles, which is, that they are not hollowed to a regular concave, but have a little projecting part in the center of the hollow; the confequence of which is, that the point of the pin will not always bear againft the fame part of the agate, and confequently the needle will not always ftand horizontal; but fometimes one end will ftand higheft, and fometimes the other, which caufes a difficulty in obferving. There is alfo another inconvenience attends it when the indices of the needle are on a level with the point of the pin, which is of more confequence; namely, that it caufes the two indices not to agree, and confequently makes a fenfible error, when only one index is made ufe of, at leaft in nice obfervations: but when the lines, ferving by way of index, are drawn on the needle itfelf, and therefore are nearly on a level with its center of gravity, it can caufe very little error. The agate cap, which was firft made for this inftrument, was of this kind; and was fo faulty, that, if no better could have been procured, it would have been neceffary either to have

 drawn

drawn the lines serving by way of index on the needle itself, or to have observed by both ends, either of which would have been attended with a considerable increase of trouble to the observer; but Mr. NAIRNE, the artist who made the instrument, has since ground some himself, which are perfectly free from this fault, the concave surface being of an extremely regular shape and well polished, and also of a very small radius of curvature; which is a matter of considerable consequence, as otherwise the point of the pin will not easily slip sufficiently near to the bottom of the hollow.

Care was taken to place the variation compass in a part of the house where it is as little likely to be affected by the attraction of the iron work as in any that could be found. As it seemed, however, to be not intirely out of the reach of the influence of that metal, I took the following method to examine how much it was influenced thereby. The instrument was removed into a large garden belonging to a house in Marlborough Street, distant from the Society's House about one mile and a quarter towards the West, where there seemed no danger of its being affected by any iron-work. Here it was placed exactly in the meridian, and compared for a few days with a very exact compass, placed in an adjoining room, and kept fixed constantly in the same situation. It was then removed back to the Society's House, and compared again with the same compass. The observations were as follow:

Observations

Obſervations made with the Society's inſtrument in the garden.

Time.		Variation by Society's Inſtrum.	Variation by Compaſs in room.	Diff.	Time.		Variation by Society's Inſtrum.	Variation by Compaſs in room.	Diff.
	h ′	° ′	° ′	′		h ′	° ′	° ′	′
1775 July 21	4 48V	21 31	21 33	— 2	1775 July 31	11 4M	21 28	21 32	— 4
	5 0	32	35	— 3		11 20	28	30	— 2
	5 26	30	28	+ 2		11 38	30	30	0
	5 43	31	32	— 1		11 57	29	32	— 3
	5 48	30	30	0		0 13V	29	33	— 4
22	10 45M	33	33	0		0 32	30	31	— 1
	11 2	29	30	— 1		2 24	32	35	— 3
	11 18	31	29	+ 2		2 54	32	31	+ 1
	11 37	31	31	0	Aug. 1	10 34M	26	28	— 2
	11 55	31	33	— 2		3 13V	32	33	— 1
	4 36V	31	32	— 1		4 33	29	29	0
	4 53	27	30	— 3		4 46	29	31	— 2
	5 22	24	26	— 2		5 12	27	29	— 2
	5 54	26	26	0		5 35	27	28	— 1
						5 57	28	30	— 2

The

The instrument being removed back to the Society's house.

Time.		Variation by Society's Instrum.	Variation by Compass in room.	Diff.	Time.		Variation by Society's Instrum.	Variation by Compass in room.	Diff.
	h ′	° ′	° ′	′		h ′	° ′	° ′	′
1775 Aug. 2	1 8V	21 45	21 32	+13	1775 Aug. 4	10 50M	21 47	21 33	+14
	1 10	44	30	+14		11 0	47	34	+13
	1 20	46	29	+17		11 10	47	35	+12
	1 30	47	29	+18		11 20	47	35	+12
	1 40	47	32	+15		11 30	46	35	+11
	1 50	47	31	+16		11 40	47	34	+13
	2 0	47	31	+16					

By a mean of the observations, the variation shewn by the compass in the room is 1′,3 greater than by the Society's instrument in the garden, and 14′,1 less than by the same instrument placed in its proper situation; so that the variation appears to be 15′,4 greater in that part of the Society's House where the compass is placed, than in the abovementioned garden; and therefore, as there is no likelihood of its being affected by any iron in the latter place, the needle seems to be drawn aside $15'\frac{1}{2}$ towards the N.W. by the iron work of the house and adjacent buildings.

On comparing the observations of the two last years together, the variation appears, after allowing for the error of the instrument, to have been 27′ greater in 1775 than in 1774; though I have been informed by Dr.

HEBERDEN,

HEBERDEN, who has made obſervations of this kind for ſeveral years paſt, that the annual alteration of the variation has, in general, been not more than 10′; and in particular, that the alteration in the laſt year appears to be only 11½′; ſo that the great difference obſerved at the Society's Houſe ſeems to be owing, not ſolely to the real alteration in the variation, but partly to ſome other cauſe; though what that ſhould be I cannot conceive, unleſs ſome change was made in the iron work either of this or the adjoining houſes between the two periods; but I do not find that any ſuch change has been made. During the laſt year, indeed, there have been two large magnets in the houſe, each conſiſting of ſeveral great bars joined together, being what the late Dr. KNIGHT uſed for making artificial magnets, and at the time of the obſervations in 1774 there was only one; but their diſtance from the compaſs is above fifty feet: and I am well aſſured, that in the ſituation in which they are actually placed, they cannot draw the needle aſide more than 3′, and not more than 15′, when the line joining their poles is placed in ſuch a direction as to act with moſt force(c). The ſingle

(c) The principle by which this was determined is, that if a magnet is placed near a variation compaſs, with its poles equi-diſtant from it, and ſituated ſo that each ſhall act equally oblique to the length of the needle, it can have no tendency to alter the variation; and that the ſituation in which it alters it moſt, except when placed nearly North or South of the compaſs, is when the line joining its poles points almoſt directly towards the needle. This experiment I tried purpoſely on the occaſion, and found it anſwer; but, I believe, any one ſkilled in magnetiſm would have granted the truth of the poſition without that precaution.

magnet

magnet in the year 1774 was placed nearly in the ſame ſituation and direction that the two were in 1775, ſo that the difference of their effect in theſe two years can hardly have been ſo much as 3′; and therefore, the great apparent alteration of the variation between the two periods cannot have been owing to them. Neither can it have been owing to the fault of the agate cap uſed in the year 1774, as the error proceeding from thence could hardly be more than 2 or 3′. It is intended that, for the future, the abovementioned magnets ſhall be kept always in the ſame ſituation and direction that they are in at preſent, and in which they were in 1775.

Of the Dipping-needle.

IN this inſtrument the ends of the axis of the needle roll on horizontal agate planes, a contrivance being applied, by which the needle is at pleaſure lifted off from the planes, and let down on them again, in ſuch manner as to be ſupported always by the ſame points of the axis reſting on the ſame parts of the agate planes; and the motion with which it is let down is very gradual and without ſhake. The general form of the inſtrument, the ſize and ſhape of the needle, and the croſs uſed for balancing it, are the ſame as in the dipping-needle deſcribed in Phil. Tranſ. vol. LXII. p. 476. It is alſo made by the ſame artiſt Mr. NAIRNE.

It may be ſeen in the Meteorological Journal, that the dip was obſerved firſt with the front of the inſtrument

to the West, and then to the East; after which the poles of the needle were reversed, and the dip observed both ways as before. The reason of this is, that the mean of the observed dips, in these four situations, differs very little from the truth, though the needle is not well balanced, and even though a great many other errors are committed in the construction of the instrument; provided the needle is made equally magnetical after the poles are reversed as before[(d)]; and that the difference of the observed dip, in these four situations, is not very great, as will appear from the following considerations.

First, let fig. 7. be a front view of the needle; AB a line parallel to the direction of magnetism therein; and CD a perpendicular thereto, meeting it in the line joining the centers of the cylindrical ends of the axis, or in the axis of motion as we may call it. If the needle was truly balanced, its center of gravity would be in *d*, the intersection of AB and CD. Suppose now, that the needle is not truly balanced, but that its center of gravity is in *g*; draw *gn* perpendicular to AB, cutting it in *m*; and let the parts *mn* and *mg* be equal. When the instrument is turned half-way round, so that the contrary face of the needle is presented towards us, the edge ADB, which is now lowest, will become uppermost, and the center of gravity will be in that situation in which the point *n* now is; therefore, the mean between the forces with which the

(*d*) It is easy to see whether the needle is made equally magnetical after the poles are reversed as before, by counting the number of vibrations which it makes in a minute.

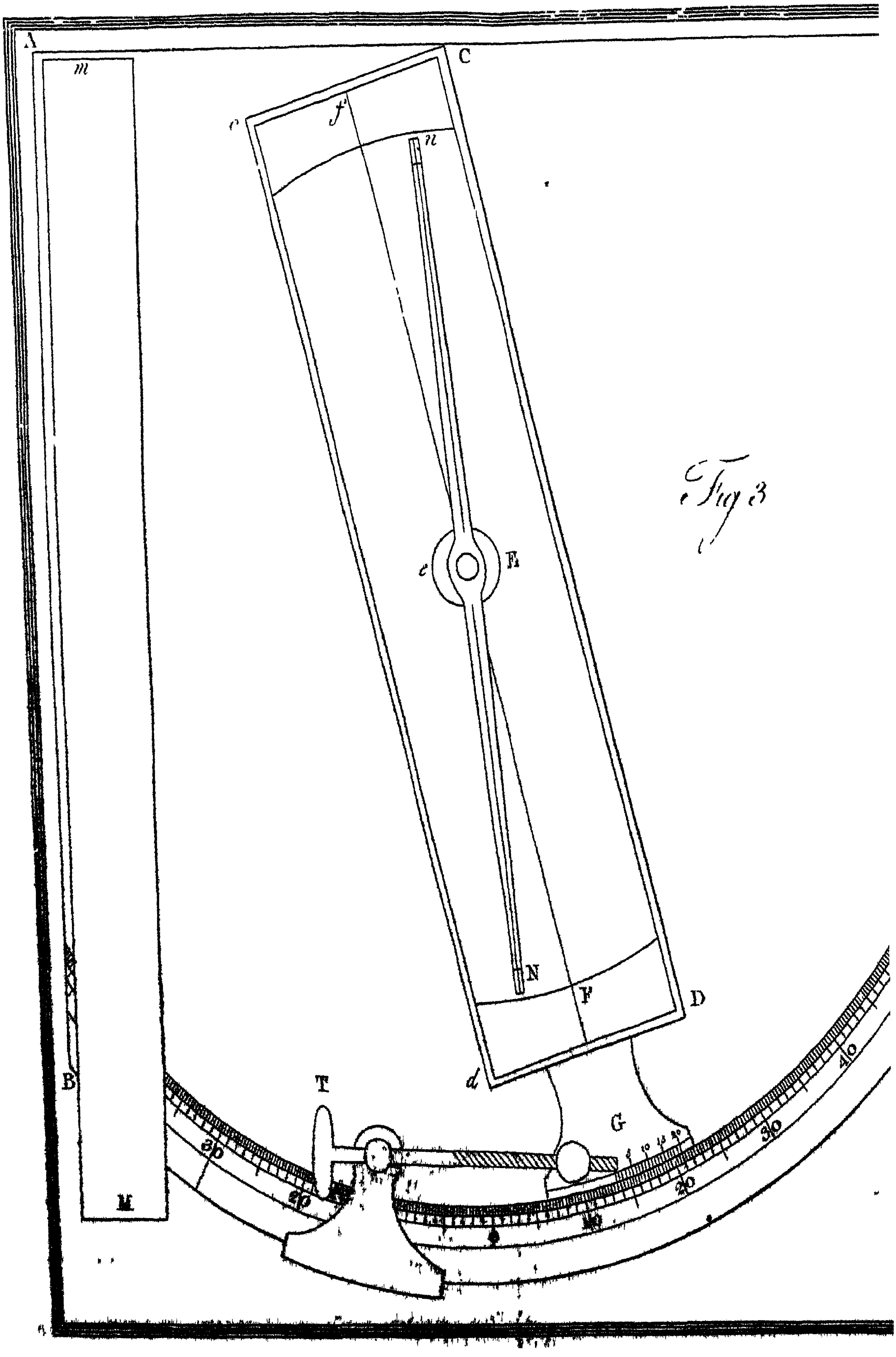
needle is drawn out of its true position in these two situations, in consequence of its not being truly balanced, is accurately the same; and the mean between the two observed dips is very nearly the same, as if the center of gravity was at *m*. But if the center of gravity is at *m*, the dip will be very nearly as much too great in the present state of the needle, as it will be too little when the poles are reversed. Therefore, the mean of the observed dips in these four situations will be very nearly the same as if the needle was truly balanced.

Secondly, if the planes on which the axis rolls are not horizontal, the dip will be very nearly as much greater than it would otherwise be, when one face is turned to the West, as it is less when the other is; for if these planes dip towards the South in one case, they will dip as much towards the North in the other, supposing the levels by which the instrument is set to remain unaltered. Consequently, the mean of the two observations will be very nearly the same as if they were placed truly horizontal.

Thirdly, by the same method of reasoning it appears, that the mean of the two abovementioned observations will be not at all altered, though the line, joining the mark on that end of the needle by which we observe, with the axis of motion, is not parallel to the direction of magnetism in the needle; that is, though the mark does not coincide with the point A or B, or though the line joining the two divisions of 90° is not perpendicular to the horizon, or though the axis of motion does not pass through the center of the divided circle, provided it is in

the ſame horizontal plane with it. If, indeed, the axis of motion is not in the ſame horizontal plane with the center of the divided circle, the error proceeding from thence will not be compenſated by this method of obſerving, unleſs both ends of the needle are made uſe of. This, however, is of no conſequence as, it is eaſy to examine whether they are in the ſame horizontal plane or not.

But the error which is moſt difficult to be avoided is, that which proceeds from the ends of the axis being not truly cylindrical. I before ſaid, that the parts of them which reſt on the agate planes are always exactly the ſame. The inſtrument is ſo contrived, however, that we may on occaſion, by giving the axis a little liberty in the notches by which it is lifted up and down, make thoſe planes bear againſt a part of the axis diſtant about $\frac{1}{100}$ or $\frac{1}{50}$th of an inch from their uſual point of bearing. Now, I find, that when the axis is confined ſo as to have none of this liberty, and when care is taken, by previouſly making the needle ſtand at nearly the right dip, that it ſhall vibrate in very ſmall arches when let down on the planes; that then, if the needle is lifted up and down any number of times, it will commonly ſettle exactly at the ſame point each time, at leaſt the difference is ſo ſmall as to be ſcarcely ſenſible; but if it is not ſo confined, there will often be a difference of 20′ in the dip, according as different parts of the axis reſt on the planes, and that though care is taken to free the axis and planes from duſt as perfectly as poſſible, which can be owing only to ſome irregularity in the axis. Moreover, if



A
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Fig 3
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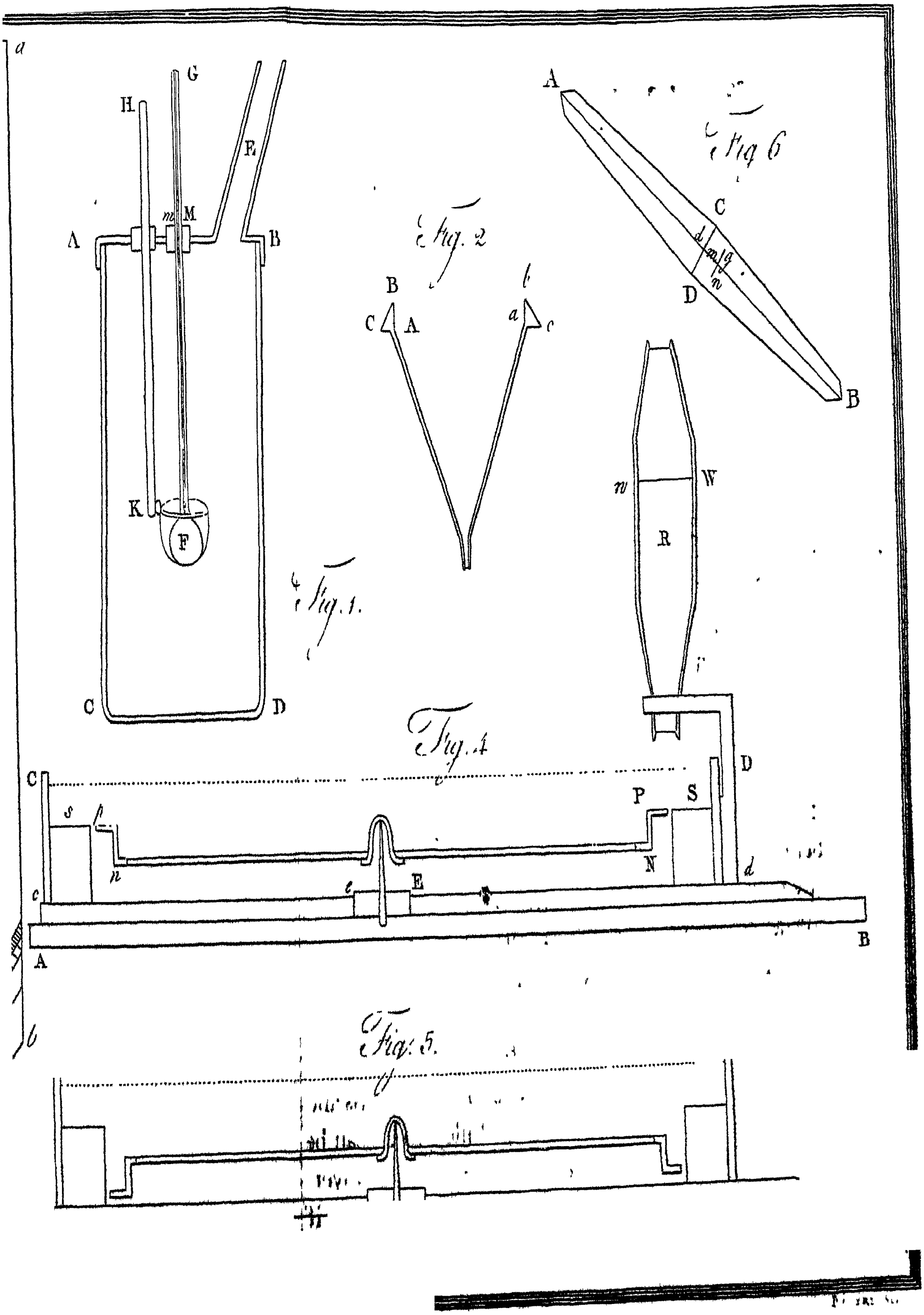
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Fig. 1.
Fig. 2
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Fig 6
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Fig. 4
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Fig. 5.

if the needle vibrates in arches of five or more degrees, when let down on the planes, there will frequently be as great an error in the dip. It is true, that the part of the agate planes, which the axis rests on when the vibrations are stopped, will be a little different according to the point which the needle stood at before it was let down; which will make a small difference in the dip as shewn by the divided circles, when only one end of the needle is observed, though the real dip or inclination of the needle to the horizon is not altered: but this difference is by much too small to be perceived; so that the above-mentioned error cannot be owing to this cause. Neither does it seem owing to any irregularity in the surface of the agate planes, for they were ground and polished with great accuracy; but it most likely proceeds from the axis slipping in the large vibrations, so as to make the agate planes bear against a different part of it from what they would otherwise do. I have great reason to think, that this irregularity is not owing either to want of care or skill in the execution, but to the unavoidable imperfection of this kind of work. I imagine too, that this instrument is at least as exact, if not more so, than any which has been yet made.

The following table contains the result of some observations which I made, partly with a view to determine the true dip at this time in a place out of reach of the influence of any iron work, and partly to see how nearly different needles would agree. The instruments were all tried in the same garden in which the variation compass was observed, and all on the 10th, 11th, 13th, and 14th

days

days of October, 1775, except that marked *, which was tried on the 15th of the preceding April.

			Poles reversed.		Poles restored to their first situation.		True Dip.
	East	West	East	West	East	West	
The Society's needle,	72 32	72 8	72 9	72 40	72 59	71 50	72 23
Another of the same construction, belonging to Mr. NAIRNE,	72 56	*72 29	71 45	73 21	72 51	72 27	72 37
One of mine on nearly the same construction,	72 33	72 22	71 41	73 23	72 34	72 18	72 30
Another needle in the same frame,	72 22	72 7	71 40	73 53	72 16	72 30	72 33
A needle of mine, made by SISSON, partly on the same construction as Mr. LORIMER's(e),	†73 1	71 49	71 57	73 0			72 27
Another of Mr. NAIRNE's on the same construction,	73 8	72 0	73 15	71 57			72 35

Each of the numbers set down in the above table is the mean of two observations, the instruments being observed first with the front to the East, then to the West; then a second time to the East, and then again to the West; and in all the observations, except those with the two last instruments, which are of a different construction, care was taken that the needle should vibrate in very small arches when let down on the agate planes. By a mean of all, the true dip at London, at this time, comes out 72° 30′, the different needles all agreeing

(e) See Phil. Transf. vol. LXV. p. 79.

within

within 14′, which is a difference considerably less than I should have expected. It appears also, that the dipping-needle, in the situation in which it is placed at the Society's House, is not much affected by any iron work, as the dip shewn by it in the garden differs only 7′ from that set down in the journal of the weather.

According to NORMAN, the inventor of the dipping-needle, the dip at London in the year 1576 was 71° 50′ (e); in 1676 it was 73° 47′, according to Mr. BOND (f); Mr. WHISTON in 1720 made it 75° 10′ (g); Mr. GRAHAM in 1723 made it between 73½ or 75° (h), his different trials varying so much; and at present it appears to be 72° 30′. I do not know how much Mr. BOND's determination is to be depended on, as he does not say by what means he arrived at it; but, I believe, Mr. WHISTON's is pretty accurate, for he observed the dip in many parts of the kingdom, and the observations agree well together; so that it is reasonable to suppose, that his instrument was a good one, and that he observed in places where the needle was not much influenced by iron work. The dip, therefore, seems to have been considerably greater about the year 1720, than it was in NORMAN's time, or is at present: it appears, however, to alter very slowly in comparison of the variation.

(e) New Attractive, c. 4. (f) Longitude found, p. 65.

(g) Longitude and Latitude found by Dipping-needle, p. 7, 49, and 94.

(h) Phil. Transf. N° 389. p. 332.

XXII. *An*

XXII. *The Method taken for preserving the Health of the Crew of His Majesty's Ship the* Resolution *during her late Voyage round the World. By Captain* James Cook, *F. R. S. Addressed to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Mile-end,
March 5, 1776.

SIR,

R. Mar. 7, 1776.

AS many gentlemen have expressed some surprize at the uncommon good state of health which the crew of the *Resolution*, under my command, experienced during her late voyage; I take the liberty to communicate to you the methods that were taken to obtain that end. Much was owing to the extraordinary attention given by the Admiralty, in causing such articles to be put on board, as either by experience or conjecture were judged to tend most to preserve the health of seamen. I shall not trespass upon your time in mentioning all those articles, but confine myself to such as were found the most useful.

We had on board a large quantity of Malt, of which was made sweet-wort, and given (not only to those men who had manifest symptoms of the scurvy, but to such also as were, from circumstances, judged to be most liable to

that

that diſorder) from one to two or three pints in the day to each man, or in ſuch proportion as the ſurgeon thought neceſſary; which ſometimes amounted to three quarts in the twenty-four hours. This is without doubt one of the beſt antiſcorbutic ſea-medicines yet found out; and if given in time will, with proper attention to other things, I am perſuaded, prevent the ſcurvy from making any great progreſs for a conſiderable time: but I am not altogether of opinion, that it will cure it in an advanced ſtate at ſea.

Sour Krout, of which we had alſo a large proviſion, is not only a wholeſome vegetable food, but, in my judgment, highly antiſcorbutic, and ſpoils not by keeping. A pound of it was ſerved to each man, when at ſea, twice a week, or oftener when it was thought neceſſary.

Portable Soup or Broth, was another eſſential article, of which we had likewiſe a liberal ſupply. An ounce of this to each man, or ſuch other proportion as was thought neceſſary, was boiled with their peaſe three days in the week; and when we were in places where freſh vegetables could be procured, it was boiled with them and with wheat or oatmeal, every morning for breakfaſt, and alſo with dried peaſe and freſh vegetables for dinner. It enabled us to make ſeveral nouriſhing and wholeſome meſſes, and was the means of making the people eat a greater quantity of greens than they would have done otherwiſe.

Further, we were provided with Rob of lemons and oranges; which the ſurgeon found uſeful in ſeveral caſes.

 Amongſt

Amongſt other articles of victualling we were furniſhed with ſugar in the room of oil, and with wheat inſtead of much oatmeal, and were certainly gainers by the exchange. Sugar, I imagine, is a very good antiſcorbutic; whereas oil, ſuch at leaſt as is uſually given to the navy I apprehend has the contrary effect. But the introduction of the moſt ſalutary articles, either as proviſion or medicines, will generally prove unſucceſsful, unleſs ſupported by certain rules of living.

On this principle, many years experience, together with ſome hints I had from Sir HUGH PALLISER, the Captains CAMPBELL, WALLIS, and other intelligent officers, enabled me to lay down a plan whereby all was to be conducted. The crew were at three watches, except upon ſome extraordinary occaſions. By this means they were not ſo much expoſed to the weather as if they had been at watch and watch: and they had generally dry cloaths to ſhift themſelves when they happened to get wet. Care was alſo taken to expoſe them as little as poſſible. Proper methods were employed to keep their perſons, hammocks, bedding, cloaths, &c. conſtantly clean and dry. Equal pains were taken to keep the ſhip clean and dry between decks. Once or twice a week ſhe was aired with fires; and when this could not be done, ſhe was ſmoaked with gunpowder moiſtened with vinegar or water. I had alſo frequently a fire made in an iron pot at the bottom of the well, which greatly purified the air in the lower parts of the ſhip. To this and cleanlineſs, as well in the ſhip as amongſt the people, too great attention

cannot be paid; the leaſt neglect occaſions a putrid, offenſive ſmell below, which nothing but fires will remove: and if theſe be not uſed in time, thoſe ſmells will be attended with bad conſequences. Proper care was taken of the ſhip's coppers, ſo that they were kept conſtantly clean. The fat, which boiled out of the ſalt beef and pork, I never ſuffered to be given to the people, as is cuſtomary; being of opinion that it promotes the ſcurvy. I never failed to take in water wherever it was to be procured, even when we did not ſeem to want it; becauſe I look upon freſh water from the ſhore to be much more wholeſome than that which has been kept ſome time on board. Of this eſſential article we were never at an allowance, but had always abundance for every neceſſary purpoſe. I am convinced, that with plenty of freſh water, and a cloſe attention to cleanlineſs, a ſhip's company will ſeldom be much afflicted with the ſcurvy, though they ſhould not be provided with any of the antiſcorbutics before mentioned. We came to few places where either the art of man or nature did not afford ſome ſort of refreſhment or other, either of the animal or vegetable kind. It was my firſt care to procure what could be met with of either by every means in my power, and to oblige our people to make uſe thereof, both by my example and authority; but the benefits ariſing from ſuch refreſhments ſoon became ſo obvious, that I had little occaſion to employ either the one or the other.

Theſe, SIR, were the methods, under the care of Providence, by which the *Reſolution* performed a voyage of

 three

three years and eighteen days, through all the climates from 52° North to 71° South, with the loſs of one man only by diſeaſe, and who died of a complicated and lingering illneſs, without any mixture of ſcurvy. Two others were unfortunately drowned, and one killed by a fall; ſo that of the whole number with which I ſet out from England I loſt only four.

I have the honour to be, SIR, &c.

Extract of a Letter from Captain Cook *to Sir* John Pringle, *dated* Plymouth Sound, July 7, 1776.

I ENTIRELY agree with you, that the dearneſs of the Rob of lemons and of oranges will hinder them from being furniſhed in large quantities, but I do not think this ſo neceſſary; for though they may aſſiſt other things, I have no great opinion of them alone. Nor have I a higher opinion of vinegar: my people had it very ſparingly during the late voyage; and towards the latter part, none at all; and yet we experienced no ill effects from the want of it. The cuſtom of waſhing the inſide of the ſhip with vinegar I ſeldom obſerved, thinking, that fire and ſmoke anſwered the purpoſe much better.

XXIII. *Extraordinary Electricity of the Atmosphere observed at* Islington *on the Month of* October, 1775. *By Mr.* Tiberius Cavallo. *Communicated by* William Watson, *M. D. V. P. R. S.*

R. Mar. 12, 1776.

BEFORE I enter on the particular narration of the observation made with an electrical kite on the 18th of last October, it will be necessary to give an idea of the scale of my quadrant electrometer used on the occasion, which, being constructed in some measure different from what are commonly sold in shops, will, no doubt, give an unsettled idea of my narration, by expressing the same intensity of electricity under different degrees from the others. In order to this, therefore, it must be observed, that, when the kite is raised, I generally connect with the end of its string a small cylindrical conductor, nine inches long and one inch diameter, made of pasteboard covered with tin-foil; with this I connect the quadrant electrometer, which shews me exactly the state, increase, and decrease of electricity. The apparatus being thus disposed, I have observed, that when the electrometer is at 10°, a little bran presented to the conductor will be attracted by it at the distance of about six-tenths of an inch; when the electrometer is at 20°, the bran will be attracted at the distance of one inch and a quarter; when at 30°, it will be

be attracted at the diſtance of two inches and one-fifth; and when at 35°, it will be attracted at the diſtance of three inches. This being premiſed, I come now to the narration of the experiment.

October the 18th, after having rained a great deal in the morning and night before, the weather grew a little clear in the afternoon, the clouds appearing ſeparated and pretty well defined; the wind was Weſt, and rather ſtrong, and the atmoſphere in a temperate degree of heat. In theſe circumſtances, at three o'clock in the afternoon, I raiſed a ſmall electrical kite, which meaſured three feet nine inches in length, and three feet in breadth, giving to it 360 feet of wired ſtring. The angle that the ſtring, or rather the cord of the incurvated ſtring, generally made with the horizon, was near 60°, and, in conſequence, the kite's perpendicular height was about 310 feet. After the end of the ſtring had been inſulated with a ſilk lace, and a leathern ball covered with tin-foil had been hanged to it, I tried the power and quality of the electricity, and found it poſitive and pretty ſtrong; in a little time a ſmall cloud paſſing over, the electricity increaſed a little; but the cloud being gone, it decreaſed again to its former degree. The ſtring of the kite now was faſtened by the ſilk lace to a poſt in the yard of the houſe where I live, which is ſituated near Iſlington; and I was amuſing myſelf and ſome other perſons with charging two coated phials, and giving ſeveral ſhocks with them. While I was ſo doing, the electricity, ſtill poſitive, began to decreaſe; and in two or three minutes time it was ſo weak, that it could be hardly

hardly perceived with a very ſenſible electrometer, made with two cork balls after Mr. CANTON's manner. Seeing at the ſame time, that a large and black cloud was approaching the zenith, which, no doubt, cauſed the decreaſe of electricity, indicating imminent rain, I introduced the end of a ſtring through a window in a firſt-floor room, wherein I faſtened it by the ſilk lace to an old chair; the quadrant electrometer was fixed upon the ſame window, and was connected by a wire with the ſtring of the kite. Being now three quarters of an hour after three o'clock, the electricity was abſolutely unperceivable: however, in two or three minutes time it began again to appear, but now, upon trial, was found to be negative; ſo that it was plain, that its ſtopping was no more than a change from poſitive to negative, which was evidently occaſioned by the approach of the cloud; part of which by this time had reached the zenith of the kite, and the rain alſo had began to fall in large drops. The cloud came farther on, the rain increaſed, and the electricity keeping pace with it, the electrometer ſoon arrived to 15°. Seeing now that the electricity was pretty ſtrong, I took again the two coated phials, and began again to charge them, and to give ſhocks to ſeveral by-ſtanders; but the phials were not charged above three or four times, before I perceived that the electrometer was arrived to 35°, and was ſtill increaſing. The ſhocks now being very ſmart, I deſiſted from charging the phials any longer; and, conſidering the rapid advances of the electrometer, thought to take off the inſulation of the ſtring,

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in caſe, that, if the electricity ſhould increaſe farther, it might be ſilently conducted to the earth, without cauſing any bad accident by being accumulated in the inſulated ſtring. To effect this, as I had no proper apparatus near me, I thought to take away the ſilk lace, and faſten the ſtring itſelf to the chair; accordingly I diſengaged the wire that connected with the electrometer, laid hold of the ſtring, untied it from the ſilk lace, and faſtened it to the chair. But while I effected this, which took up leſs than half a minute, I received about a dozen or fifteen very hard ſhocks, which I felt all along my arms, in my breaſt and legs, ſhaking me in ſuch a manner, that I had hardly power enough to effect my purpoſe, and to warn the people in the room to keep their diſtance. As ſoon as I took my hand off of the ſtring, the electrical fluid, in conſequence of the chair being a bad conductor, began to ſnap between the ſtring and the ſhutter of the window, which was the neareſt body to it. The ſnappings, which were audible at a good diſtance out of the room, ſeemed at firſt *iſochronous* with the ſhocks I had received; but in about a minute's time they became more frequent, ſo that the people of the houſe compared their ſound to the rattling noiſe of a jack going when the fly is off. The cloud was now juſt over the kite; it was black and pretty well defined, of almoſt a circular form, its diameter appearing to be about 40°. The rain was copious, but not remarkably heavy. As the cloud was going off, the electrical ſnappings began to weaken, and in a ſhort time became unaudible. I went then near the ſtring, and finding the electricity weak,

 but

but ftill negative, I infulated it again, thinking to keep the kite up fome time longer; but, as another larger and denfer cloud was approaching apace towards the zenith, and I had then no proper apparatus to prevent bad accidents, I refolved to pull the kite in. Accordingly, a gentleman, who was by me, began pulling it in, while I was winding up the ftring. The other cloud was now very nearly over the kite; and the gentleman who was pulling in the ftring told me, that he had received one or two flight fhocks in his arms; and that, if he were to receive one more, he would certainly let the ftring go. Upon which I laid hold of the ftring, and pulled the kite in as faft as I could, without any farther obfervation. When the kite was pulled in, it was ten minutes after four o'clock; fo that all the time that this experiment took up was one hour and ten minutes. There was neither thunder or lightning perceived that day, nor indeed for fome days before or afterwards.

XXIV. *Proposals for the Recovery of People apparently drowned.* *By* John Hunter, *Esq. F. R. S.*

R. Mar. 21, 1776.

HAVING been requested by a principal member of the society, lately established for the recovery of persons apparently drowned, to commit my thoughts on that subject to paper; I readily complied with his request, hoping, that, although I have had no opportunities of making actual experiments upon drowned persons, it might be in my power to throw some lights on a subject so closely connected with the inquiries which, for many years, have been my favourite business and amusement. I therefore collected together my observations and experiments relative to the loss and recovery of the actions of life, and shewed them to a Society of which I am a member; who approved of them as new and curious, and unanimously recommended their being submitted to the judgement of this learned Body. The practice is new, and has furnished as yet few important and clear facts. If we judge of the question by our general knowledge of the animal œconomy, I am afraid, it is so imperfectly understood, that our reasoning from it alone could not be relied on: nevertheless, on a subject so interesting to humanity, we must not be idle; we must throw out our observations, and reason as well as we can from

from the few *data* we have, in hopes that the fubject, thus put fairly into the hands of the publick, may in time, by their united endeavours, become perfectly underftood.

I fhall confider an animal, apparently drowned, as not dead; but that only a fufpenfion of the actions of life has taken place. This, probably, is the cafe in the beginning of all violent deaths, except thofe caufed by lightning or electricity, by which abfolute death may be produced inftantaneoufly.

How a blow on the ftomach caufes death I have not been able to afcertain. In all thofe cafes which have fallen under my obfervation, the concomitant circumftances have been fuch as alfo attend death caufed by electricity; *viz.* a total and inftantaneous privation of fenfe and motion without convulfions, and confequently without any fucceeding rigor of mufcles, totally differing in thefe circumftances from death, where the perfon is ftruck fenfelefs by any injury done to the brain. I fhould confider the fituation of a perfon drowned to be fimilar to that of a perfon in a trance. In both the action of life is fufpended, without the power being deftroyed; but I am inclined to believe, that a greater proportion of perfons recover from trances than from drowning; becaufe a trance is the natural effect of a difpofition in the perfon to have the action of life fufpended for a time; but drowning being produced by violence, the fufpenfion will more frequently laft for ever, unlefs the power of life is roufed to action by fome applications of art. That I

 may

may more fully explain my ideas upon this ſubject, it will be neceſſary to ſtate ſome propoſitions.

First, that ſo long as the animal retains the powers, though deprived of the action, of life, the cauſe of that privation may frequently be removed; but, when the powers of life are deſtroyed, the action ceaſes to be recoverable. Secondly, it is neceſſary to mention that I conſider part of the living principle as inherent in the blood (a). The laſt propoſition I have to eſtabliſh is, that the ſtomach ſympathizes with every part of an animal, and that every part ſympathizes with the ſtomach; therefore, whatever acts upon the ſtomach as a cordial, or rouſes its natural and healthy actions, and whatever affects it, ſo as to produce debility, has an immediate effect upon every part of the body. This ſympathy is ſtrongeſt with the vital parts. Beſides this univerſal ſympathy between the ſtomach and all parts of the body, there are peculiar ſympathies; for inſtance, the heart ſympathizes immediately with the lungs. If any thing is received into the lungs, which is a poiſon to animal life, ſuch as the volatile part in the burning of charcoal, volatile vitriolic acid, and many other well known ſubſtances, the motion of the heart immediately ceaſes, much ſooner than if the

(a) That the living principle is inherent in the blood, is a doctrine which the nature of this paper will not allow me at preſent to diſcuſs; thus much, however, it may be proper to ſay, that it is founded on the reſults of many experiments. But it may be thought neceſſary here, to give a definition of what I call the living principle: ſo far as I have uſed that term in this paper, I mean, to expreſs that principle which preſerves the body from diſſolution with or without action, and is the cauſe of all its actions.

trachea

trachea had been tied; and from experiments it appears, that any thing ſalutary to life, applied to the lungs, will reſtore the heart's action after it has been at reſt ſome time.

I ſhall divide violent deaths into three kinds. Firſt, where only a ſtop is put to the action of life in the animal; not, however, by any irreparable injury to a vital part. If this action is not reſtored in a certain time, it will be irrecoverably loſt. The length of that time is ſubject to conſiderable variation; which probably depends on circumſtances, we are at preſent unacquainted with. The ſecond is, where an injury is done to a vital part: as, by taking away blood till the powers of action are loſt, by a wound or preſſure on the brain or ſpinal marrow; notwithſtanding which, there remains ſufficient life in the ſolids, if actions could be reſtored to the vital parts. The third is, where abſolute death inſtantly takes place in every part, which is often the caſe in ſtrokes of lightning; in the common method of killing eels, by throwing them on ſome hard ſubſtance, in ſuch manner as that the whole length of the animal ſhall receive the ſhock at the ſame inſtant; and, as I believe, by a blow on the ſtomach; in all which caſes the muſcles remain flexible (b).

(b) On the other hand, when an eel is killed by chopping it into a number of pieces, the powers of life are by thoſe means rouſed into action; and, as every part dies in that active ſtate, every part is found ſtiff after death. This explains the cuſtom of cutting fiſh into pieces while yet alive, in order to make them hard, uſually known by the name of crimping.

Nor

Now, the prefent confideration is, which of the kinds of violent death drowning comes under? I think, it comes under the firft; and upon that ground I fhall confider the fubject.

The lofs of motion in drowning feems to arife from the lofs of refpiration, and the immediate effects which this has upon the other vital motions of the animal; at leaft, this privation of breathing appears to be the firft caufe of the heart's motion ceafing; therefore, moft probably, the reftoration of breathing is all that is neceffary to reftore the heart's motion: for if a fufficiency of life ftill exifts to produce that effect, we may fuppofe every part equally ready to move the very inftant in which the action of the heart takes place, their actions depending fo much upon it. What makes it very probable, that the principal effect depends upon air being thrown into the lungs, is, that children in the birth, when too much time has been fpent after the lofs of that life which is peculiar to the *fœtus*, lofe altogether the difpofition for the new life. In fuch cafes there is a total fufpenfion of the actions of life, the child remains to all appearance dead, and would die, if air were not thrown into its lungs, and the firft principle of action by thefe means reftored. To put this in a ftill clearer light, I will give the refult of fome experiments which I made, in the year 1755, upon a dog.

A pair of double bellows were provided, conftructed in fuch a manner as by one action to throw frefh air into the lungs, and by another to fuck out again the air which

 had

had been thrown in by the former, without mixing them together. The muzzle of theſe bellows was fixed into the *trachea* of a dog, and by working them he was kept perfectly alive. While this artificial breathing was going on, I took off the *ſternum* of the dog, and exposed the lungs and heart; the heart continued to act as before, only the frequency of its action was conſiderably increaſed. I then ſtopped the motion of the bellows, and the heart became gradually weaker, and leſs frequent in its contraction, till it left off moving altogether: by renewing my operation, the heart began again to move, at firſt very faintly, and with longer intermiſſions; but, by continuing the artificial breathing, the motion of the heart became again as frequent and as ſtrong as before. This proceſs I repeated upon the ſame dog ten times, ſometimes ſtopping for five, eight, or ten minutes. I obſerved, that, every time I left off working the bellows, the heart became extremely turgid with blood, and the blood in the left ſide became as dark as that in the right; which was not the caſe when the bellows were working. Theſe ſituations of the animal appear to me exactly ſimilar to drowning.

The loſs of life in drowned people has been accounted for, by ſuppoſing that the blood, damaged by want of the action of the air in reſpiration, is ſent, in that vitiated ſtate, to the brain and other vital parts; by which means the nerves loſe their effect upon the heart, and the heart in conſequence its motion. This, however, I am fully convinced is falſe: firſt, from the experiments on the dog,

dog; in whose case a large column of bad blood, *viz.* all that was contained in the heart and pulmonary veins, was pushed forward, without any ill effect being produced; and next, from the recovery of drowned persons and still-born children, which, under those circumstances, never could happen, unless a change of the blood could take place in the brain, prior to the restoration of the heart's motion: therefore, the heart's motion must depend immediately upon the application of such air to the lungs, and not upon the effects which air has upon the blood, and which that blood has upon the vital parts. These are only secondary operations in the animal œconomy.

It frequently happens in the case of drowning, that assistance cannot be procured till a considerable time after the accident; every moment of which delay renders recovery more precarious, the chances of which are not only diminished in the parts where the first powers of action principally reside, but also in every other part of the body.

In offering my sentiments on the method of treating people who are apparently drowned, I shall lay before you, first, what I would recommend to have done; secondly, what I would wish might be avoided.

When assistance is called in, soon after the immersion, perhaps blowing air into the lungs may be sufficient to effect a recovery (c). But if a considerable time, such as

(c) Perhaps the dephlogisticated air, described by Dr. PRIESTLEY, may prove more efficacious than common air. It is easily procured, and may be preserved in bottles or bladders.

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an hour, has been loſt, it is moſt probable that this will not be ſufficient; the heart, in all likelyhood, will by this time have loſt its nice connection with the lungs. It will, therefore, be proper to apply ſtimulating medicines, ſuch as the vapour of volatile alkali, mixed with the air; which may eaſily be done, by holding ſpirits of harts-horn in a cup under the receiver of the bellows. I would adviſe the air and volatile alkali to be thrown in by the noſe, if by both noſtrils ſo much the better, as we know, that applications of this kind to the olfactory nerves rouſe the living principle, and put the muſcles of reſpiration into action, while ſome applications to the mouth rather depreſs than rouſe, by producing ſickneſs. If during this operation the *larynx* be gently preſſed againſt the *œſophagus* and ſpine, it will prevent the ſtomach and inteſtines being too much diſtended by the air, and leave room for the application of more effectual *ſtimuli* to thoſe parts. This preſſure, however, muſt be conducted with judgement and caution, ſo that the *trachea* and the aperture into the *larynx* may both be left perfectly free. While this buſineſs is going on, an aſſiſtant ſhould prepare bed-cloaths, carefully brought to the proper degree of heat. I conſider heat as congenial with the living principle; increaſing the neceſſity of action it increaſes action; cold, on the other hand, leſſens the neceſſity, and of courſe the action is diminiſhed; to a due proportion of heat, therefore, the living principle owes its vigour.

From obſervations and experiments it appears to be a law of nature in animal bodies, that the degree of heat ſhould bear a proportion to the quantity of life; as life is weakened, this proportion requires great accuracy, while greater powers of life allow it greater latitudes[(d)].

I was led to make theſe obſervations by attending to perſons who are froſt-bitten; the effect of cold, in this inſtance, is that of leſſening the living principle. The powers of action remain as perfect as ever, and heat is the only thing wanting to put theſe powers into action; yet this heat muſt at firſt be gradually applied, and proportioned to the quantity of the living principle; but, as life increaſes, you may increaſe the degree of heat. If this method is not obſerved, and too great a degree of heat is at firſt applied, the perſon or part loſes entirely the living principle, and mortification enſues. This proceſs invariably takes place with regard to men. The ſame thing, I am convinced, happens to other animals. If an eel, for inſtance, is expoſed to a degree of cold, ſufficiently intenſe to benumb him till the remainder of life in him is ſcarcely perceptible, keep him ſtill in a cold of about 40°, and this ſmall quantity of life will remain for a conſiderable time without diminution or increaſe; but, if he is put into about 60°, the animal will at firſt ſhew ſtrong ſigns of returning life, but will die in a few minutes. Nor is this circumſtance peculiar to the

(d) It is upon theſe principles that cold air is found of ſo much ſervice to people who are much reduced by diſeaſe, as the confluent ſmall-pox, fevers, &c. *viz.* diminiſhing heat in proportion to the diminution of life.

diminution

diminution of life by cold. The ſame *phænomena* take place in animals who have been very much reduced by hunger.

If a lizard, or ſnake, when it goes to its autumnal hiding place, is not ſufficiently fat, the living powers become, before the ſeaſon permits it to come out, very conſiderably weakened, and perhaps ſo much as not to be again reſtored. If thoſe animals, in ſuch a ſtate, are expoſed to the Sun's rays, or placed in any ſituation which, by its warmth, would give vigour to thoſe of the ſame kind, which are poſſeſſed of a larger ſhare of life, they will immediately ſhew ſigns of increaſed life, but quickly ſink under the experiment and die; while others, reduced to the ſame degree of weakneſs, as far as appearances can diſcover, will live for many weeks, if kept in a degree of cold proportioned to the quantity of life they poſſeſs.

I obſerved many years ago, in ſome of the colder parts of this iſland, that, when intenſe cold had forced black-birds or thruſhes to take ſhelter in out-houſes, any of thoſe that had been caught, and from an ill-judged compaſſion expoſed to a conſiderable degree of warmth, died very ſoon. The reaſon of this I did not then conceive; but I am now ſatisfied, that it was owing, as in the other inſtances, to the degree of heat being increaſed too ſuddenly for the proportion of life remaining in the animal.

From theſe facts it appears, that warmth cauſes a greater exertion of the living powers than cold; and that an animal, in a weakly ſtate, may be obliged by it to exert

 a quantity

a quantity of the action of life fufficient to deftroy the very powers themfelves. (e) The fame effects probably take place even in perfect health. It appears, from experiments made in a hot room, which were read to this Society, that a perfon in health, expofed to a great degree of heat, found the actions of life accelerated fo much as to produce faintnefs and debility.

If bed-cloaths are put over the perfon fo as fcarce to touch him, fteam of volatile alkali, or of warm balfams and effential oils, may be thrown in, fo as to come in contact with many parts of his body. It will certainly prove advantageous if the fame fteams can be conveyed into the ftomach, as that feat of univerfal fympathy will by thefe means be roufed. This may be done by a hollow *bougie* and a fyringe; but this operation fhould be performed with all poffible nimblenefs, becaufe the inftrument, by continuing in the mouth, may produce ficknefs, an effect I fhould chufe to avoid. Some of the ftimulating fubftances, which are of a warm nature, and have an immediate effect, may be thrown into the ftomach in a fluid ftate; *viz.* fpirits of hartfhorn, peppermint water, juice of horfe-raddifh; as many others alfo, which produce a more lafting *ftimulus*, as balfams and turpentines, fuch as are found to quicken the pulfe of a man in health; but the quantity muft be fmall, as they have a tendency to produce ficknefs. The fame fteam and fubftances fhould alfo be thrown up by the *anus*.

(e) It is upon this principle that parts mortify in confequence of inflammation.

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The proceſs recommended under the firſt head of treatment ſhould ſtill be continued; while that recommended under the ſecond is put in practice, as the laſt is only an auxiliary to the firſt. The firſt, in many caſes, may ſucceed alone; but the ſecond without the firſt muſt, I think, always fail in caſes where the powers of life are conſiderably weakened. Motion poſſibly may be of ſervice, it may at leaſt be tried; but, as it has leſs effect than any other of the uſually preſcribed *ſtimuli*, it ſhould be the laſt part of the proceſs *(f)*. I would recommend the ſame care to the operator in regulating the proportion of every one of theſe means as I did before in the application of heat; poſſibly every one of them may have the ſame property of deſtroying intirely the feeble action which they have excited, if adminiſtered in too great a quantity; inſtead, therefore, of increaſing and haſtening the operations on the firſt ſigns of returning life being obſerved, as uſually done, I ſhould wiſh them to be leſſened, that their increaſe afterwards may be graduated, as nearly as poſſible, by the quantity of powers as they ariſe. If the heart begins to move, I would leſſen my application of air to the lungs, and with great attention obſerve when the muſcles of reſpiration began to act, that, when they do, I might leave a great deal to them. I would by

(f) How far electricity may be of ſervice, I know not; but it may, however, be tried, when every other method has failed. I have not mentioned injecting ſtimulating ſubſtances directly into the veins, though it might be ſuppoſed a proper expedient; becauſe, in looking over my experiments on that ſubject, I found none where animal life received increaſe.

all

all means forbid bleeding, which, I think, weakens the animal principle, and leffens life itfelf, confequently leffens both the powers and difpofitions to action. I would avoid introducing any thing into the ftomach, which ordinarily produces *naufea* or vomiting, as that alfo will have a fimilar effect. I would avoid likewife throwing any thing in by the *anus*, which might tend to an evacuation that way, as every fuch evacuation alfo tends to leffen the animal powers; of courfe, I have avoided fpeaking of the fumes of tobacco, which always produce ficknefs or purging, according as they are applied.

Whoever is appointed by the fociety for the purpofes of recovering drowned people, fhould have an affiftant, well acquainted with the methods intended to be made ufe off; that, while the one is going on with the firft and moft fimple methods, the other may be preparing other means, fo that no time may be loft between the operations; and the more fo, as the firft will, in all cafes, affift the fecond, and both together may often be attended with fuccefs, though each feparately might have failed.

A proper apparatus alfo is as effentially neceffary to the inftitution, a defcription of which I here annex. Firft, a pair of bellows, fo contrived with two feparate cavities, that by opening them, when applied to the noftrils or mouth of a patient, one cavity will be filled with the common air, and the other with air fucked out from the lungs, and by fhutting them again, the common air will be thrown into the lungs, and that fucked

out of the lungs dicharged into the room. The pipe of theſe ſhould be flexible, in length a foot or a foot and a half, and at leaſt three-eighths of an inch in width; by this the artificial breathing may be continued, while the other operations, the application of the *ſtimuli* to the ſtomach excepted, are going on, which could not conveniently be done, if the muzzle of the bellows were introduced into the noſe. The end next the noſe ſhould be double, and applied to both noſtrils. Secondly, a ſyringe, with a hollow *bougie*, or flexible *catheter*, of ſufficient length to go into the ſtomach, and convey any ſtimulating matter into it, without affecting the lungs. Thirdly, a pair of ſmall bellows, ſuch as are commonly uſed in throwing fumes of tobacco up the *anus*.

I ſhall conclude this paper by propoſing, that all, who are employed in this practice, be particularly required to keep accurate journals of the means uſed, and the degrees of ſucceſs attending them; whence we may be furniſhed with facts ſufficient to enable us to draw concluſions, on which a certain practice may hereafter be eſtabliſhed.

XXV. *An extraordinary Cure of wounded Intestines. By* Charles Nourse, *Surgeon, at* Oxford.

TO GEORGE SCOTT, ESQ. F. R. S.

Oxford,
Dec. 20, 1775.

SIR,

R. Mar. 29, 1776.

IN compliance with your request, and in discharge of the promise I made to you, when I had last the pleasure of seeing you in Oxford, I transmit to you, the case of JAMES LANGFORD, the unfortunate lad after whose welfare you were pleased, out of your great humanity, often to enquire. If any hints may be drawn from this narrative, that may prove useful to the young practitioner, it will not fail to give me much pleasure; if not, I shall always think that time has been well bestowed which has been employed in executing your command. Should the account I have given of this singular case prove worthy the notice of your learned Society, I shall esteem it as my greatest honour.

I am, &c.

THE

THE CASE OF JAMES LANGFORD (a).

IN the evening of the 26th of September, 1775, I was called, in great haſte, to JAMES LANGFORD, a young man in the twenty-firſt year of his age, who had been maliciouſly ſtabbed, with a knife, in the left ſide of his belly. The wound was between two and three inches in length, running from the left *os ilium* obliquely upwards towards the navel. I found him lying on the floor, weltering in his blood, with a large portion of his inteſtines forced through the wound; and I learnt, from the unhappy youth himſelf, that, as ſoon as the wound was inflicted, the bowels began to appear; and, by the time I got to him, which could not exceed ten minutes, I verily believe, that the full half of the inteſtinal tube was protruded through the opening. This I attributed, in ſome meaſure, to the fulneſs of the ſtomach; for, immediately before the accident happened, he had eaten a very hearty ſupper. The wound at firſt bled freely; but the hæmorrhage was ſoon reſtrained by the preſſure of the prolapſed inteſtines, which were, to a great degree, diſtended with air; and from this circumſtance I was flattered with the beſt hopes that they had eſcaped the aſſaſſin's knife; but, to my great diſappointment, it proved otherwiſe, as will appear moſt evidently from the ſequel of this narrative. Examining his pulſe, I found it

(a) He is a journeyman book-binder, and lives with Mr. ALEXANDER THOMPSON, in Oxford.

was exceedingly low, quick, and interrupted; his ſkin was all over cold and clammy, and he laboured under great languor, anxiety, and pain about the *præcordia*. He likewiſe complained of a diſagreeable tingling and numbneſs of the whole thigh, leg, and foot, of the ſide wounded; and acquainted me, that he dropped on the floor in conſequence of the inability of the limb to ſupport him, and not from any faintneſs, as might have been reaſonably expected from the loſs of blood, or through fear, to which, indeed, he ſeemed an utter ſtranger. I ordered him to be conveyed to his bed in an horizontal poſture, leſt the raiſing of the body might encourage a farther deſcent of the parts which ſtill remained in the *abdomen*; and a fomentation of port wine with warm water to be got ready immediately, out of which a double flannel ſhould be wrung, and applied directly to the prolapſed inteſtines, and renewed occaſionally, to prevent them from getting too dry, as well as to preſerve, as much as poſſible, their natural heat. The reduction of the diſplaced bowels was begun with laying the patient's legs over an aſſiſtant's ſhoulders, who was deſired to kneel upon the bed for that purpoſe, with his back towards him, and then the legs were brought forward as far as to the hams. By this means the lower parts of the body were elevated, and, in conſequence, the weight of the bowels falling back towards the cheſt, counteracted their further protruſion. While the patient continued in this poſition, I endeavoured, with my hands, to force the guts back into their proper place; but ſoon

found, from the quantity of them protruded, together with their great inflation, that a larger or more extended pressure than my own hands could afford me was necessary; and not thinking it prudent to employ any of the by-standers in so hazardous a task, lest by their inexperience they might handle the bowels too roughly, I sent for two of my fellow-labourers in the care of the Radcliffe Infirmary, to my assistance. As soon as they came, the reduction was again attempted; one of us directing that portion of the bowel which was last protruded, while the two others made a gentle, regular, and circumscribed pressure from all sides towards the opening. But this endeavour not succeeding, convinced us, that it would be much safer to enlarge the wound, to facilitate the return of the prolapsed parts, than hazard the necessity of handling them too much, or exposing them too long to the external air, either of which would, in all probability, have proved fatal. This being done accordingly, by continuing the wound in the same direction upwards about two inches, the exposed bowels were easily and soon returned into the *abdomen*. We then brought the edges of the wound together, and kept them by the suture called *gastroraphia*, leaving a proper opening in the most depending part of it for the discharge of the blood or matter which might be collected in the cavity; and afterwards it was dressed in the usual way, lightly and almost superficially, with an anodyne poultice over all.

The regimen enjoined him, with respect to diet, was only gruel, panado, and sage-tea, with barley water or thin gruel to drink; and the medicines were the following:

℞ *Mannæ,*
Ol. Amygd. D. ana ℥ss.
Aq. Alexit. Simp. ℥j.
— *Nucis Moschat.* ʒj. *m. f. haustus quam primum sumendus et quartâ quâque horâ repetendus donec alvus responderit.*

℞ *Decoct. Pectoral.* ℥viij.
Ol. Lini. ℥ij.
Tinct. Thebaic. guttas xl. *f. enema quovis tempore infundendum si dolor abdominis urgeat.*

27th, Visiting him early the next morning, I found the night had been spent in great restlessness and inquietude, notwithstanding the clyster had been thrown up according to the direction. He was exceedingly low; his skin felt still cold and clammy; his pulse weak and fluttering; he complained of frequent chills, and an oppressive tightness of his belly, though the wound had discharged considerably a thin, serous humour, which had wetted the bandage quite through. Nor was the tension of the *abdomen*, at this time, sufficient to account for this oppressive pain he complained of; from whence I concluded it to be spasmodic. The dressings were removed; and I desired my apprentice, to foment the part with an infusion of the

emollient

emollient flowers for a full hour, and to take particular care that the ſtupes were applied of a very moderate warmth; often having obſerved, that this manner of applying them, when an inflammation was either to be reſolved or prevented, was more effectual than when the heat has been greater. This obſervation, upon a little reflection, will be found agreeable to reaſon; for as great heat proves an aſtringent, on the contrary, a moderate and kindly warmth relaxes, and, by promoting a free perſpiration of the parts to which it is applied, ſooner effects the end propoſed. The wound was dreſſed as before, with the addition of two ounces of the ſpecies *pro cataplaſmate de cymino* to the poultice; and as the draughts he had taken had not produced any motion of the bowels, it was thought proper to inject the underwritten clyſter, as ſoon as it could be prepared:

℞ *Decoct. pro Clyſt.* ℥viij.
Ol. Lini. ℥ij.
Elect. Lenitiv.
Mel. Solutiv. ana ℥ſſ. *f. enema.*

This in about half an hour occaſioned a very copious diſcharge of *fæces*, together with a good deal of blood; ſome of it congealed into lumps, the reſt fluid. This circumſtance did not fail to alarm my apprehenſion of the imminent danger of the lad's ſituation, as it was no longer to be doubted, but that the bowels were wounded in ſome part of them; but what part ſtill remained a matter of conjecture. When the clyſter had done operating, he took this draught:

℞ *Sperm.*

℞ *Sperm. Ceti (Mucilag. Gum. Arab. Solu.)* ℈j.
Aq. Alexit. Simp. ℥j ß.
Ol. Amygd. D.
Syr. de Meconio, ana ℥ij.
Tinct. Thebaic. guttas x. *M. f. haust.*

Late this evening the fomentation and dreſſings were again renewed, and directions given, that he ſhould take one of the draughts, with manna, oil, &c. as firſt preſcribed, at three o'clock in the morning; and to repeat them regularly every fourth hour, till they had had their deſired effect.

28th, He had got but little ſleep in the night, though he had lain ſomething quieter, with ſhort, but interrupted ſlumbers intervening. His pulſe, and all the other ſymptoms, were much in the ſame ſtate as yeſterday, except a general ſoreneſs of the *abdomen*; of which, at this time, he made great complaint, and more particularly about the wounded part. The whole belly was full and tenſe; and, when I ſtruck it with my finger, it returned an *emphyſematoſe* ſound. The diſcharge from the wound was increaſed; it had ſtained the bandage of a deep reddiſh-brown colour, and was of a diſagreeable ſmell. The draughts he had taken had not yet moved him; therefore, I deſired they might be continued, according to the general direction; and that, in caſe any ſtools ſhould come off, to put them by, ſeparately, for my inſpection. By the time I made my evening viſit, he had had two motions; in the firſt, there was a good deal of fluid blood; with the laſt, but little, no more than juſt to give it a tinge.

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He was evidently relieved by the evacuation; was calmer and more compoſed; his pulſe was rather more up, and his ſkin warmer. He ſaid, he found himſelf litheſomer; that he was not ſo tight, and thought he breathed with more freedom. When I came to looſen the bandage, I was greatly ſurprized to find it daubed all over with the diſcharge; but, as ſoon as the dreſſing was removed, there was no evidence wanting to aſſure me, that this diſcharge was in part *fœcal*, not only from the colour and ſmell of it, but likewiſe from the ſharp pain it had occaſioned in paſſing through the wound. My hopes of his recovery now began to fail me; however, I reſolved to perſevere, and act as though I was ſure of ſucceſs. After dreſſing, he was ordered to take the anodyne draught, and to begin again the manna draughts with oil early in the morning.

29th, Before I came to viſit him, he had had another motion; and the nurſe informed me, that his night had been better than any of the preceding ones, he having ſlept, at different times, full three hours. His pulſe was ſtronger, but remitting, and his ſkin inclining to perſpire. The tongue was foul, and the water clear and pretty high-coloured. In the ſtool, which had come off this morning, I did not find any blood, or in any he had afterwards during the time of his confinement. The wound had diſcharged a great deal, and was more inflamed; and the edges of it looked thick and ill-natured, and were ready to ſeparate from each other. The tenſion of the belly ſtill kept up, though I did not perceive, that it

had

had at all increafed. The opening draughts were continued, once in fix hours only, through the courfe of this day, which kept him fufficiently open; and the anodyne was repeated at ten o'clock this night.

30th, This morning things wore but a melancholy afpect. His night had been reftlefs, and his head confufed, and he talked fometimes incoherently; his pulfe was increafed, though exceedingly irregular, and the fkin felt hot and dry; he was thirfty, and complained of a great tightnefs, particularly about the region of the ftomach; his countenance was hollow, the eyes being funk, with a deadnefs in them not eafily to be expreffed. The wound had difcharged very much, and it was extremely offenfive. The edges of it were inverted, much fwollen, and feparated from each other confiderably more than the preceding day. He likewife complained of a fharp, burning pain, deep in the wound, but could not exprefs precifely where. As foon as the wound was dreffed, the anodyne clyfter was adminiftered; and I defired, he might have a fmall bafon of the infufion of mint, with a knob of fine fugar, got ready for him as foon as poffible, and that he would fip it down as warm as he could. At two o'clock this afternoon he was feized fuddenly with a moft violent vomiting, and brought up a large quantity of bile. This I the more wondered at, as he had never made the leaft complaint of ficknefs, or *naufea*, from the time of his accident; for every thing he had taken had fat eafy and well upon his ftomach. What he had brought up was of fo dark a colour, that I

imagined

imagined it was mixed with blood; but, upon a careful examination of it, found I was miſtaken. When the vomiting was over, the nurſe gave him a little more of the mint infuſion; and, ſoon after, he fell into a ſound ſleep, which continued more than an hour. In the evening he was hot and uneaſy, complaining of thirſt, and a pain in his head; his pulſe was increaſed, and his ſkin felt dry. The wound had made a prodigious diſcharge, which I obſerved always to increaſe, in proportion, as the bowels were more or leſs looſened by the medicines he was taking; and, from the violent efforts of the *abdominal* muſcles in the time of his vomiting, moſt of the ſtitches in the wound were broken, ſo that you might plainly ſee into the cavity of the *abdomen*. After dreſſing the wound, twelve ounces of blood were taken from the arm, and the anodyne draught was given to him ſoon after.

Oct. 1ſt, I learnt, from the people about him, that for a few hours, after he had taken the *opiate*, he lay compoſed; but, ſoon after mid-night, he awaked in great hurry and confuſion, complaining of his ſtomach and bowels, accompanied with convulſive twitchings of the tendons; and that, about five o'clock this morning, he brought up another large quantity of bile, which gave him great relief; for afterwards he lay perfectly eaſy, and got between two and three hours ſleep. At nine o'clock, when I made my morning viſit, I found him much refreſhed, and without any kind of complaint. His pulſe was full, but much ſteadier than it had been

any time before, and his ſkin was open. The water he had made was turbid, though ſtill high-coloured. The wound, indeed, made but an indifferent appearance; the edges of it were very ſloughy, particularly the tendons of the oblique muſcle, and ſo far receded from each other as to make it neceſſary to divide the remaining ſtitches. The lower part of the wound, or that next to the *ilium*, was beginning to digeſt, and the inflammation and tenſion of the belly to abate. The opening draughts, made a little warmer, were continued, which kept the bowels conſtantly and gently open. In the evening his pulſe was rather increaſed; and I found that, ſome time in the afternoon, he had brought up a little more bile, though without any previous complaining. After dreſſing, I directed more blood to be drawn, and the *opiate* to be repeated.

2d, The nurſe acquainted me this morning, that her patient had had a very quiet night, and had ſlept many hours without intermiſſion; that he had taken a ſufficient quantity of nouriſhment, and that it had ſat well on his ſtomach. I found him chearful, without any complaint, except that of hunger. His pulſe was ſteady, his ſkin ſoft and open, his tongue getting cleaner, and his water beginning to break. The diſcharge, this morning, from the ſore was exceedingly offenſive; and when I had taken off the dreſſing, I was really aſtoniſhed at the horrid appearance! The wound was burſt open, in ſuch a manner as to aſſume a circular form, and was rather more than three inches in the leaſt diameter of it.

it. In the baſe of this dreadful opening, there was nothing to be ſeen but the circumvolutions of the ſmall guts; and how this amazing breach was to be reſtored, I could not eaſily conceive. Had any one taken a view of the wound at this time, who was unacquainted with the real progreſs of it, he muſt naturally have concluded, there had been a great loſs of ſubſtance. The patient was dreſſed with thin pledgets of very fine unformed lint, moiſtened with the oil of the flowers of the *hypericum* luke-warm, laid firſt upon the expoſed bowels; afterwards the cavity was filled up lightly with the ſame ſort of lint dry; the edges were covered with a moderately warm digeſtive, and the whole ſecured with the uniting bandage; which bandage had been uſed from the very beginning, to prevent, as much as art could prevent, the impending miſchief.

3d, Appearances this morning were very favourable; he had ſlept well moſt part of the night; his pulſe was perfectly quiet, and his ſkin moderately open. The water was become better coloured, and had made a fair ſeparation; ſo that, from this time, all ſigns of fever, inflammation, and pain its concomitant, intirely ceaſed: nor did there even ariſe any alarming, or even diſagreeable ſymptom afterwards; but every thing went on in an eaſy, regular way. The wound digeſted kindly, and was conſtantly dreſſed twice a day, as the quantity, and indeed the quality, of the diſcharge from it required. The opening medicines were repeated occaſionally, and his nights ſecured by a few drops of the Theban tincture.

In a few days, the ſloughs from the edges of the *abdominal* muſcles ſeparated, and left the ſore ſo largely open, that I could eaſily diſcover from whence the *fæces* made their *exit*, which was from the middle of that part of the *colon* that lies between the left kidney, to which it is attached, and the upper part of the *ſacrum*, where it empties itſelf into, and forms the *rectum*.

It was exceedingly ſatisfactory and pleaſing to obſerve, from day to day, the progreſs Nature made in renovating this formidable breach, and her mean of accompliſhing it; for, after a little time, the ſurface of the inteſtines looked florid, and began to pullulate, throwing up ſmall grains of fleſh from every point. Theſe *granules*, daily increaſing, united with each other, and after filling up the intervals between the circumvolutions of the bowels, became one uniform ſurface; which ſurface meeting with that of the raw edges of the integuments, they both adhered together, and became one continued ſore. As the wound incarned, the *fæcal* diſcharge leſſened daily, and about the twenty-ſecond or twenty-third day intirely ceaſed. I now allowed him chicken broth, milk porridge, calves-feet jelly, &c. The wound was dreſſed once a day with dry lint only, and in ſeven weeks it was completely healed.

XXIII. *Extract*

XXIII. *Extract of a Letter from Mr.* Alexander Small, *Surgeon to the Train of Artillery at* Minorca, *to Sir* John Pringle, *Bart. P. R. S. Dated* St. Philip's, *Aug.* 8, 1775.

R. Apr 15, 1776.

I BEG leave to give you this trouble, and to add ſome conjectures, which may be a kind of addition to Dr. CLEGHORN's Account of this Iſland. I live near the Glacis, and the artillery men are conſtantly lodged in the ſquare within the caſtle. A little beyond the ſquare is our hoſpital. My attendance at theſe two places gives me no more than a ſalutary exerciſe. The artificers in the civil branch of the ordnance, who are under my care, live near me in the remains of the town of St. Philip. I call them remains, becauſe many of the houſes were deſtroyed during the late ſiege, and many ſince; it being determined, that the town ſhall be removed to a greater diſtance from the caſtle. A new ſpot is accordingly marked out near Engliſh Cove, on the ſide of the harbour; and barracks for two regiments, together with ſome houſes, are already built. The new as well the old town are built in a very dry ſituation, on a ſolid rock, on which there is not a drop of ſtagnating water, nor is there any near the ſurface of the earth; for the water the inhabitants have for uſe, is either rain-water

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kept in ciſterns, or water drawn out of wells, from twenty to ſixty or more feet in depth; nor are there any marſhes near either town, or indeed in this part of the iſland.

The caſtle of St. Philip ſtands, or rather is cut out of the ſolid rock, on a promontory, two-thirds of which are waſhed by the ſea, and is open to the ſea winds from two-thirds of the compaſs. As there is no tide there is no ſlimy ſhore, which might ſend forth putrid vapours at low-water; and if there were a tide, our ſhore is one continued rock, on which there is not any putreſcent ſubſtance. Indeed the rocks are ſo free from filth, that after a ſtrong wind has raiſed the ſea-water, and carried it into cavities hollowed in the rock by ſtorms, it drys there into pure white ſalt.

During the hot weather in July, Auguſt, and September, our unhealthy ſeaſon, the air is daily ventilated, either by general winds, which paſs freely over the iſland, or by ſea-breezes. The air over the land being rarefied by the reflected rays of the Sun, and by being in contact with the heated earth, neceſſarily makes room for the cooler and denſer air in contact with the cooler ſea-water. Whence, in ſuch a ſituation, ſhall we ſeek for the cauſes of *tertianas*, ſo called here, and ſo much dreaded during the hot months? Two cauſes ſeem to offer themſelves; one very obvious, the other rather more remote. The Southerly winds are much complained of here, as occaſioning a general laſſitude, and as bringing with them noxious *effluvia* from Africa; but whoever conſiders the diſtance

diſtance between this iſland and Africa, will ſcarcely believe, that the air can carry with it, ſo far, any other quality than the warmth attending the ſeaſon of the year. Gibraltar, nearer Africa, and more Southerly than we are, is not ſubject to *tertians*, nor are ſome places even in this iſland. The cauſes therefore muſt be ſought for on the ſpot. In a ſituation, ſuch as I have deſcribed ours to be, you may believe, that ſhade and a plentiful ſupply of freſh ſucculent culinary plants muſt be very deſirable. On ſo dry a rock, an artificial ſupply of moiſture muſt become neceſſary, eſpecially in a country where there ſeldom is rain from May to October. It is not an eaſy matter to keep a due mean in the uſe of whatever experience ſhews to be neceſſary. If a little does good, we are apt to conclude, that a great deal will do more good: thus, I think, it fares with us in regard to the uſe of water in our gardens. In order to have a garden, it is neceſſary here to have a draw-well. The drawing of water is the labour of an aſs; and, as the labour is not hard, the beaſt is kept at it pretty conſtantly, and thus plenty of water is drawn up. As the water is hard, and is much colder than the temperature of the air, it is kept in ciſterns for ſome time, expoſed to the Sun, till it acquires the temperature of the air, and thus becomes more friendly to vegetation than if uſed immediately on being drawn up. Having thus obtained plenty of water, they beſtow it moſt copiouſly on their gardens. Suppoſe yourſelf landed at St. Philip's in this ſeaſon of the year, on a dry, parched rock, and that you were told, that the rock was uniformly the ſame all the

way

way to Mahon, a diftance of two miles, and that you were under a neceffity of going to Mahon in the evening; would you expect to be ferenaded on this rock with the croaking of frogs all the way you went? This literally is the cafe. The gardens on each fide the road are fo much watered, that the frogs, bred in the cifterns which contain the water, fpread and enjoy themfelves around, and frequently take up their abode in trees. This fhews that even the trees abound much in watery juices, feeing the exhalations arifing from them yield an atmofphere agreeable to the frogs. Where land is thus abundantly watered and clofely planted with fucculent vegetables, many parts of thefe vegetables, as well as the infects which feed on them, will be liable to putrify; and a putrid vapour may be thence exhaled in the evening efpecially, and during the night, when there feldom is wind to carry them off. Wherever the inhabitants can find a proper depth of mould, within a convenient diftance of a market, fo many fources of putrid exhalations are formed.

Let me give an inftance, to fhew that this opinion is not merely ideal. On the North-fide of St. Philip's there is a road, bounded on the North by a wall, called the Line-wall: Dr. HUCK muft remember it and the environs. Along and near that wall there are many gardens, which thus fend forth unhealthy vapours; and the effect is, that the houfes on the South-fide of that road, though facing the North, and thereby, one would think, the healthier, are called Rotten-row, their unhealthinefs

being

being owing, as is believed, to the vapours ariſing from theſe gardens; for houſes ſituated on the South-ſide of the town, though at a little diſtance, are by much the more healthy, though more expoſed to the heat of the meridian Sun. Other inſtances might be quoted; and experience has taught us, that it is found to be very prejudicial to health to remain expoſed to the evening dews near Mahon or St. Philips, round which theſe gardens chiefly abound, while country peaſants lie in their vineyards whole nights without being hurt, the vines being left to nature for a ſupply of moiſture.

Perhaps it may ſeem, that, while the heat and drought of the climate makes this method of gardening neceſſary, and at the ſame time require a large ſupply of ſucculent vegetables and fruits, theſe ill conſequences muſt be unavoidable. But M. DE CHATEAU-VIEUX, a magiſtrate of Geneva, has pointed out a very promiſing remedy, ſuggeſted to him by ſome of his own judicious experiments in agriculture.

The ſecond general cauſe of *tertians* was pointed out to me by Dr. MUNRO, phyſician of this iſland, an ingenious gentleman, and very obſervant of every thing relating to his profeſſion.

The rocks of this iſland conſiſt chiefly of two kinds of ſtone; one ſo hard that ſcarce any tool can touch it; and the other ſo ſoft, that it is eaſily cut into any form. It much reſembles the Bath ſtone, and is called Cantoon ſtone. The firſt is impervious to any fluid; but the other ſucks up, or is penetrated by, moiſture, like filtering

 ſtones.

ſtones. I found, ſome years ago, this to be the caſe with the Bath ſtone. Mr. ALLEN covered his workmen's houſes with flat ſtones brought from the quarry, and cut of a proper thickneſs: I mentioned to one of the inhabitants, that their houſes being ſo cloſely covered were, I ſuppoſed, very warm; and was ſurprized at being told, that they were much the reverſe; for that in rainy weather, the water penetrated through the ſtones; and that in froſty weather, the inſide of the roofs were covered with ice, whereby their upper rooms were of little uſe to them. Theſe different qualities of the ſtones in this iſland are not, perhaps, ſufficiently attended to.

When houſes are built on the hard rock, all within the walls is levelled; and on that floor the poorer inhabitants live. As this ſtone takes a greater degree of cold than ſubſtances leſs ſolid, and does not ſo ſoon come to the temperature of the air; it conſequently cools, and attracts to it the moiſture in the air, and retains it long on its ſurface. In order to avoid the damp cold feel, if the inhabitants can afford to buy a mat, they cover the floor with it; under which the wet remaining, induces a degree of putrefaction, which renders the houſes more unhealthy, and reduces the inhabitants to a ſtate ready to be affected by any diſtemper, eſpecially by the *tertian*, which ſpreads by contagion. As the moiſture remaining on this ſtone is but temporary, provided there are drains to carry the water off, its bad effects are eaſily prevented by keeping a fire burning, or by laying the ground-floor with terrace, or with deal-boards.

When houſes are built on the ſoft Cantoon ſtone, the rain that falls without ſoaks through it; and if there are no means of carrying it off, it remains in the ſtone, becomes putrid, gradually exhales, and thus becomes highly prejudicial to health. I might quote ſeveral inſtances of families dying in conſequence of ſuch putrid moiſture; but ſhall reſt ſatisfied with one, becauſe it became an object of general obſervation.

At a little diſtance from, and to the Northward of the Line-wall, a lofty building was erected for a houſe of entertainment. The people who inhabited it became very unhealty; and in a few years ſo much ſo, that two or three whole families died in it. This houſe, I am told, is built on Cantoon ſtone, the hollows filled with Cantoon rubbiſh, and is ſurrounded by gardens continually watered, ſome of which are higher than the floor of this building; by which means the ſtone became the receptacle of the waſte water. In order to remedy this inconveniency, the floor was taken up, and a ſtench aroſe which the workmen could ſcarcely bear, and changed the colour of every metallic ſubſtance about them. People were impreſſed with ſo ſtrong a prejudice againſt the houſe, that it remains uninhabited and an uſeleſs building. The ſame has happened in other dwelling-houſes; in which the ſame ſtench, and other indications of putrefaction, were met with, as in the former caſe.

I can yet, thank God, ſay very little of the diſeaſe of this country, for we ſtill continue very healthy. I have met with two inſtances of how faſt a hold *tertians* take

 of

of the conſtitution, when the patients have been ſubject to frequent relapſes. Dr. MUNRO tells me, that an addition of *Myrrh* ℨij. to *Cort. Peru.* ℥j. is the beſt medicine he knows in this caſe. I have tried it in both, with ſeeming ſucceſs; for we can ſay nothing certain on that head till the winter. One of them was much afflicted with pains in the lower part of the belly, even after the fever had left him. I gave him *Calomel.* gr. vi. *Pulv. Rhei* gr. xv. in a bolus, and repeated it, at proper intervals, four times. He thinks his preſent health is much owing to it; as does my other patient from the ſame medicine. I have found *calomel* peculiarly beneficial to ſeveral children who had periodical heats on them, eſpecially at night.

XXVI. *Of*

XXVI. *Of the Tides in the* South Seas. *By Captain* James Cook, *F. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Mile-End,
April 2, 1776.

SIR,

R. Apr. 18, 1776.

IN compliance with your requeſt, I ſend you my obſervations on the tides in Endeavour River, on the Eaſt Coaſt of New Holland, in latitude 15° 26′ S.

About 11 o'clock in the evening of the 10th of June 1770, as we were ſtanding off ſhore, the ſhip ſuddenly ſtruck, and ſtuck faſt on a reef of coral rocks, about ſix leagues from the land. At this time I judged it was about high water, and that the tides were taking off, or decreaſing, as it was three days paſt the full Moon; two circumſtances by no means in our favour. As our efforts to heave her off, before the tide fell, proved ineffectual, we began to lighten her, by throwing over-board our guns, ballaſt, &c. in hopes of floating her the next high-water; but, to our great ſurprize, the tide did not riſe high enough to accompliſh this by near two feet. We had now no hopes but from the tide at midnight; and theſe only founded on a notion, very general indeed among ſeamen,

ſeamen, but not confirmed by any thing which had yet fallen under my obſervation, that the night-tide riſes higher than the day-tide. We prepared, however, for the event, which exceeded our moſt ſanguine expectations; for, about 20 minutes after 10 o'clock in the evening, which was a full hour before high-water, the ſhip floated. At this time the heads of rocks, which on the preceding tide were, at leaſt, a foot above water, were wholly covered. I was fully ſatisfied with the truth of the remark, after getting into the river, where we remained from the 17th of June till the 4th of Auguſt, repairing the damage the ſhip had received. As this was to be done with the aſſiſtance of the tides, it led me to make the following obſervations, which upon any other leſs important occaſion might have eſcaped my notice.

The times of high-water on the full and change days I found to be about a quarter after nine; the evening-tide, at the height of the ſpring, to riſe nine feet perpendicular, the morning-tide ſcarce ſeven; and the low-water preceding the higheſt or evening-tide, to fall or recede conſiderably lower than the one preceding the morning-tide. This difference in the riſe and fall of the tide was uniformly the ſame on each of the three ſprings which happened while we lay in the place, and was apparent for about ſix or ſeven days; that is, for about three days before and after the full or change of the Moon. During the neep, the tide was very inconſiderable, and if there was any difference between the riſe of

of the tide in the day and in the night, it was not obſerved; but to the beſt of my recollection none was perceptible. Excepting two or three mornings, when we had a land-breeze for a few hours, we had the winds from no other direction than S.E., which is the ſame as this part of the coaſt, and from which quarter I judged the flood-tide came. The wind, for the moſt part, blew a briſk gale, and rather ſtronger during the day than the night. How far this laſt circumſtance might affect the evening-tide, I ſhall not pretend to determine; nor can I aſſign any other cauſe for this difference in the riſe and fall of the tide, and therefore muſt leave it to thoſe who are better verſed in this ſubject than,

SIR, your, &c.

XXVII. *An Experimental Examination of the Quantity and Proportion of Mechanic Power necessary to be employed in giving different Degrees of Velocity to Heavy Bodies from a State of Rest. By Mr.* John Smeaton, *F. R. S.*

R. April 25, 1776.

ABOUT the year 1686 Sir ISAAC NEWTON first published his *Principia*, and, conformably to the language of mathematicians of those times defined, that "the quantity of motion is the "measure of the same, arising from the velocity and "quantity of matter conjointly." Very soon after this publication, the truth or propriety of this definition was disputed by certain philosophers, who contended, that the measure of the quantity of motion should be estimated by taking the quantity of matter and the square of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increases of velocity are acquired by given bodies, when unresisted by a medium; thus gravity causes a body, in obeying its impulse during one second of time, to acquire a velocity which would carry it uniformly forward, without any additional impulse, at the rate of 32 ft. 2 in. *per* second; and if gravity is suffered to act upon it for two seconds, it

will have, in that time, acquired a velocity that would carry it, at an uniform rate, juſt double of the former; that is, at the rate of 64 ft. 4 in. *per* ſecond. Now, if in conſequence of this equal increaſe of velocity, in an equal increaſe of time, by the continuance of the ſame impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the ſame impelling power for a double time; this will be co-incident with Sir ISAAC NEWTON's definition abovementioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever cauſe, the impreſſion it will make upon an uniformly reſiſting medium, or upon uniformly yielding ſubſtances, will be as the maſs of matter of the moving body, multiplied by the ſquare of its velocity: the queſtion, therefore, properly is, whether thoſe terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been eſteemed ſynonymous, are with the moſt propriety of language to be eſteemed equal, double, or triple, when they have been generated by an equable impulſe, acting for an equal, double, or triple time; or that it ſhould be meaſured by the effects being equal, double, or triple, in overcoming reſiſtances before a body in motion can be ſtopped? For, according as thoſe terms are underſtood in this or that way, it will neceſſarily follow, that the *momenta* of equal bodies will be as the velocities, or as the ſquares of the velocities reſpectively; it being certain, that,

that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumſtances that attend the application of it, the ſame concluſion, in point of computation, will reſult. I ſhould not, therefore, have thought it worth while to trouble the Society upon this ſubject, had I not found, that not only myſelf and other practical artiſts, but alſo ſome of the moſt approved writers, had been liable to fall into errors, in applying theſe doctrines to practical mechanics, by ſometimes forgetting or neglecting the due regard which ought to be had to theſe collateral circumſtances. Some of theſe errors are not only very conſiderable in themſelves, but alſo of great conſequence to the public, as they tend greatly to miſlead the practical artiſt in works that occur daily, and which often require very great ſums of money in their execution. I ſhall mention the following inſtances.

DESAGULIERS, in his ſecond volume of Experimental Philoſophy, treating upon the queſtion concerning the forces of bodies in motion, after taking much pains to ſhew that the diſpute, which had then ſubſiſted fifty years, was a diſpute about the meaning of words; and that the ſame concluſion will be brought out, when things are rightly underſtood, either upon the old or new opinion, as he diſtinguiſhes them; among other things, tells us, that the old and new opinion may be eaſily reconciled in this inſtance: that the wheel of an underſhot water-mill is capable of doing quadruple work when the velocity of the water is doubled, inſtead of

double work only; " becauſe (the adjutage being the " ſame), ſays he, we find, that as the water's velocity is " double, there are twice the number of particles of " water that iſſue out, and therefore the ladle-board is " ſtruck by twice the matter, which matter moving with " twice the velocity that it had in the firſt caſe, the " whole effect muſt be quadruple, though the inſtan- " taneous ſtroke of each particle is increaſed only in a " ſimple proportion of the velocity." See vol. II. Annotations on lecture 6th, p. 92.

Again, in the ſame volume, lecture 12th, p. 424. referring to what went before, he tells us, " The know- " ledge of the foregoing particulars is abſolutely neceſ- " ſary for ſetting an underſhot wheel to work; but the " advantage to be reaped from it would be ſtill gueſs- " work, and we ſhould be ſtill at a loſs to find out the " utmoſt it can perform, if we had not an ingenious " propoſition of that excellent mechanic M. PARENT, of " the Royal Academy of Sciences, who has given us a " *maximum* in this caſe, by ſhewing, that an underſhot " wheel can do the moſt work, when its velocity is equal " to the third part of the velocity of the water that " drives it, &c. becauſe then two-thirds of the water is " employed in driving the wheel with a force propor- " tionable to the ſquare of its velocity. If we mul- " tiply the ſurface of the adjutage or opening by the " height of the water, we ſhall have the column of wa- " ter that moves the wheel. The wheel thus moved will " ſuſtain on the oppoſite ſide only four-ninths of that

 " weight,

"weight, which will keep it in equilibrio; but what it "can move with the velocity it goes with, will be but "one-third of that weight of equilibrium; that is, $\frac{4}{27}$ths "of the weight of the firſt column, &c.—This is the "utmoſt that can be expected."

The ſame concluſion is likewiſe adopted by MACLAURIN, in art. 907. p. 728. of his Fluxions, where, giving the fluxionary deduction of M. PARENT'S propoſition, he ſays, "that if A repreſents the weight which would "balance the force of the ſtream, when its velocity is "*a*; and U repreſents the velocity of the part of the en-"gine, which it ſtrikes when the motion of the machine "is uniform, &c.—the machine will have the greateſt "effect when U is equal to $\frac{a}{3}$; that is, if the weight that "is raiſed by the engine be leſs than the weight which "would balance the power, in the proportion of 4 to 9, "and the *momentum* of the weight is $\frac{4Aa}{27}$."

Finding that theſe concluſions were far from the truth; and ſeeing, from many other circumſtances, that the practical theory of making water and wind-mills was but very imperfectly delivered by any author I had then an opportunity of conſulting(a); in the year 1751 I began a courſe

(a) BELIDOR, *Architecture Hydraulique*, greatly prefers the application of water to an underſhot mill, inſtead of an overſhot; and attempts to demonſtrate, that water applied underſhot will do ſix times more execution than the ſame applied overſhot. See vol I. p. 286. While DESAGULIERS, endeavouring to invalidate what had been advanced by BELIDOR, and greatly preferring an overſhot

a courſe of experiments upon this ſubject. Theſe experiments, with the concluſions drawn from them, have already been communicated to this Society, who printed them in vol. LI. of their Tranſactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir GODFREY COPLEY, from the hands of our very worthy Preſident the late Earl of MACCLESFIELD. Thoſe experiments and concluſions ſtand uncontroverted, ſo far as I know, to this day; and having ſince that time been concerned in directing the conſtruction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement, I have always found between theſe two, appears to me fully to eſtabliſh the truth of the principles upon

ſhot to an underſhot, ſays, Annotat. on lecture 12. vol. II. p. 532. that from his own experience, "a well-made overſhot mill ground as much corn in the "ſame time with ten times leſs water;" ſo that betwixt BELIDOR and DESAGULIERS here is a difference of no leſs than 60 to 1.

Again, BELIDOR, vol. II. p. 72. ſays, that the center of gravity of each ſail of a wind-mill ſhould travel in its own circle with one-third of the velocity of the wind; ſo that, taking the diſtance of this center of gravity from the center of motion at 20 feet, as he ſtates it p. 38. art. 849. the circumference will be exceeding 126 feet Engliſh meaſure: a wind, therefore, to make the mill go twenty turns *per* minute, which they frequently do with a freſh wind and all their cloth ſpread, would require the wind to move above eighty miles an hour; a velocity perhaps hardly equalled in the greateſt ſtorms we experience in this climate.

which

which they were conftructed, when applied to great works, as well as upon a fmaller fcale in models.

Refpecting the explanatory deduction of DESAGULIERS in the firft example abovementioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is fhewn, Phil. Tranf. vol. LI. p. 120, 121, and 123. part 1. maxim 4. that, where the velocity of water is double, the adjutage or aperture of the fluice remaining the fame, the effect is eight times; that is, not as the fquare but as the cube of the velocity; and the fame is inveftigated concerning the power of the wind arifing from difference of velocity, p. 156. being part 3. maxim 4.

The conclufion in the fecond example abovementioned, adopted both by DESAGULIERS and MACLAURIN, is not lefs wide of the truth than the foregoing; for if that conclufion were true, only $\frac{4}{27}$ths of the water expended could be raifed back again to the height of the refervoir from which it had defcended, exclufively of all kinds of friction, &c. which would make the actual quantity raifed back again ftill lefs; that is, lefs than one-feventh of the whole; whereas it appears, from table 1. p. 115. of the faid volume, that in fome of the experiments there related, even upon the fmall fcale on which they were tried, the work done was equivalent to the raifing back again about one quarter of the water expended; and in large works the effect is ftill greater, approaching towards half, which feems to be the limit for the underfhot mills, as the whole would be the limit for

the overſhot mills, if it were poſſible to ſet aſide all friction, reſiſtance from the air, &c. ſee p. 130.

The velocity alſo of the wheel, which, according to M. PARENT's determination, adopted by DESAGULIERS and MACLAURIN, ought to be no more than one-third of that of the water, varies at the *maximum* in the above-mentioned experiments of table I. between one third and one half; but in all the caſes there related, in which the moſt work is performed in proportion to the water expended, and which approach the neareſt to the circumſtances of great works, when properly executed, the *maximum* lies much nearer to one half than one third; one half ſeeming to be the true *maximum*, if nothing were loſt by the reſiſtance of the air, the ſcattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminiſh the effect more, at what would be the *maximum* if theſe did not take place, than they do when the motion is a little ſlower.

Finding theſe matters, as well as others, to come out in the experiments, ſo very different from the opinions and calculations of authors of the firſt reputation, who, reaſoning according to the Newtonian definition, muſt have been led into theſe errors from a want of attending to the proper collateral circumſtances; I thought it very material, eſpecially for the practical artiſt, that he ſhould make uſe of a kind of reaſoning in which he ſhould not be ſo liable to miſtakes; in order, therefore, to make this matter perfectly clear to myſelf, and poſſibly ſo to others, I reſolved

I resolved to try a set of experiments from whence it might be inferred, what proportion or quantity of mechanical power is expended in giving the same body different degrees of velocity. This scheme was put in execution in the year 1759, and the experiments were then shewn to several friends, particularly my very worthy and ingenious friend Mr. WILLIAM RUSSELL.

In my experimental inquiry concerning the powers of water and wind before referred to, I have, p. 105. part 1. defined what I meant by power, as applied to practical mechanics, that is, what I now call mechanical power; which, in terms synonymous to those there used, may be said to be measured by multiplying the weight of the body into the perpendicular height from which it can descend; thus the same weight, descending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight descending from the same height is also a double power, because it likewise is capable of producing a double effect; and a given body, descending through a given perpendicular height, is the same power as a double body descending through half that perpendicular; for, by the intervention of proper levers, they will counter-balance one another, conformably to the known laws of mechanics, which have never been controverted. It must, however, be always understood, that the descending body, when acting as a measure of power, is supposed to descend slowly, like the weight of a clock or a jack; for, if quickly descending,

it is ſenſibly compounded with another law, *viz.* the law of acceleration by gravity.

DESCRIPTION OF THE MACHINE.

AB is the baſe of the machine placed upon a table.

AC is a pillar or ſtandard.

CD is an arm, upon the extremity of which is fixed a plate *fg*, which is here ſeen edge-ways, through which is a ſmall hole for receiving a ſmall ſteel pivot *e*, fixed in the top of the upright axis *e*B; the lower end of this axis finiſhes in a conical ſteel point, which reſts upon a ſmall cup of hard ſteel poliſhed at B.

HI is a cylinder of white fir, which paſſes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of ſliding

KL two cylindric weights of lead of equal ſize, which are capable of being fixed upon any part of the cylindric arms, from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal diſtances from the center, and the axis perpendicular, the whole will be balanced upon the point at B, and moveable thereupon by an impelling power, with very little friction.

Upon the upper part of the axis are formed MN, two cylindrical barrels, whereof M is double the diameter of N; they have a little pin ſtuck into one ſide of each at *o*, *p*.

Q is a piece capable of ſliding higher or lower, as occaſion requires; and carries

R a light pulley of about three inches diameter, hung upon a ſteel axis, and moveable upon two ſmall pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one ſide, ſo as to point (at a mean) between the ſurface of the bigger barrel and the leſs.

S is a light ſcale for receiving weights, and hangs by a ſmall twine, cord, or line, that paſſes the pulley, and terminates either upon the bigger barrel or the leſs, as may be required; the ſliding-piece Q being moved higher or lower for each, that the line, in paſſing from the pulley to the barrel, may be nearly horizontal. The end of the line, that is furtheſt from the ſcale, is terminated by a ſmall loop, which hangs on upon the pin *o*, or the pin *p*, according as the bigger or the leſſer barrel is to be uſed.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the ſcale S, it is obvious, that it will cauſe the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulſe of the weight in the ſcale; and when the line is wound off to the pin, the loop ſlips off, and the ſcale then falling down, the weight will ceaſe to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except ſo far as it muſt be gradually leſſened by the friction of the machine and reſiſtance of the air, which being ſmall, the bodies will revolve ſometime before their velocity is apparently diminiſhed.

MEASURES

MEASURES OF SOME PARTS OF THE MACHINE.

	inches.
Diameter of the cylinders of lead, or the heavy bodies,	2,57
Length of ditto,	1,56
Diameter of the hole therein,	,72

Weight of each cylinder 3 lbs. Avoirdupois.

Greater diſtance of the middle of each body from the center of the axis,	8,25
The ſmaller diſtance of ditto,	3,92
10 turns of the ſmaller barrel raiſes the ſcale, 5 ditto of the bigger ditto,	25,25

When the bodies are at the ſmaller diſtance above ſpecified from the axis of rotation, they are then in effect at half the greater diſtance from that axis; for, ſince the axis itſelf, and the cylindric arms of wood, keep an unvaried diſtance from the center of rotation, the bodies themſelves muſt be moved nearer than half their former diſtance, in order that, compounded with the unvariable parts, they may be virtually at the half diſtance. In order to find this half diſtance nearly, I put in an arm of the ſame wood, that only went through the axis, without extending in the oppoſite direction; one of the bodies being put upon the end of this arm, at the diſtance of 8,25 inches, the whole machine was inclined till the body and arm became a kind of pendulum, and vibrated

at the rate of 92 times *per* minute; and as a pendulum of the half length vibrates quicker in the proportion of $\sqrt{1}$ to $\sqrt{2}$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the ſame inclination of the machine, the weight was moved upon the arm till it made 130 vibrations *per* minute; which was found to be, when it was at 3,92 inches diſtance from the center as above ſtated, which is about $\frac{2}{10}$ths of an inch nearer than the half diſtance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjuſted ready for uſe; and with it were tried the following experiments, each of which was repeated ſo many times as to be fully ſatisfactory.

TABLE OF EXPERIMENTS.

No	Ounces Avoirdupois in the Scale.	Barrel ufed, M the bigger, N the fmaller.	The Arms, W the whole, H the Half-length.	Number of Turns of the Line wound on the Barrel.	Time of the Defcent of the Weight in the Scale.	Time in making 20 Revolutions with equable Motion.
					$''$	$''$
1	8	M	W	5	$14\frac{1}{4}$	29
2	8	N	W	10	$28\frac{1}{4}$	$29\frac{1}{4}$
3	8	N	W	$2\frac{1}{2}$	$14\frac{1}{4}$	$58\frac{1}{2}$
4	32	M	W	5	7	14
5	32	N	W	10	14	$14\frac{3}{4}$
6	32	N	W	$2\frac{1}{2}$	7	$28\frac{3}{4}$
7	8	M	H	5	7	$14\frac{3}{4}$
8	8	N	H	10	14	15
9	8	N	H	$2\frac{1}{2}$	7	$30\frac{1}{4}$
1	2	3	4	5	6	7

The $58''\frac{1}{2}$ in number 3, column 7, was determined in fact from $29''\frac{1}{4}$, being the time of making 10 equable revolutions, after the weight was dropped off, in order to prevent the fenfible retardation that might take place, and affect the obfervation, if continued for 20 revolutions made fo flowly.

FURTHER DEFINITIONS.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be neceſſary alſo to define the following terms:

Impulſe or Impulſion,
Impulſive Force or Power,
Impelling Force or Power,
By all which, I underſtand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be meaſured either by its being a weight of itſelf, or by being counter-balanced by a weight. It may alſo act either immediately upon the body to be moved, ſo that if motion is the conſequence, they move with the ſame velocity; and that, either by a ſimple contact, or by being drawn as by a cord, or puſhed as by a ſtaff: or it may act by the intervention of a lever or other mechanic inſtrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers muſt be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a ſtandard length of lever, which is the method taken in the ſubſequent reaſoning upon the preceding experiments. An impelling power, therefore, conſiſting of a double weight, or requiring a double weight to counter-balance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intenſity.

OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING EXPERIMENTS.

1ſt, By the firſt experiment it appears, that the mechanic power employed, conſiſting of 8 ounces in the ſcale, deliberately deſcending (by 5 turns of the bigger barrel) through a perpendicular ſpace $25\frac{1}{4}$ inches, will repreſent the quantity of mechanic power which cauſes the two heavy bodies, from a ſtate of reſt, to acquire a velocity, ſuch as to carry them equably through 20 circumferences of their circle of revolution in the ſpace of 29″; and that the time in which the mechanic power produced this effect was $14''\frac{1}{4}$, as appears by column 6th. And this mechanic power we ſhall expreſs by the number 202, the product of the number of ounces in the ſcale multiplied by the inches in its perpendicular deſcent, for $8 \times 25\frac{1}{4} = 202$.

2d, By the ſecond experiment, as 10 turns of the ſmaller barrel are equal to the ſame perpendicular height as 5 turns of the bigger, it follows, that the ſame mechanic power, *viz.* 202, acting upon the ſame heavy bodies to accelerate them, produces the very ſame effect in generating motion in the bodies as it did before, *viz.* 20 revolutions in $29''\frac{1}{4}$, the ſmall difference of $\frac{1}{4}$ of a ſecond being no more than may reaſonably be attributed to the unavoidable errors ariſing from friction of the machine, want of perfect accuracy in its meaſures, reſiſtance of the air, and imperfections in the obſervations themſelves, which muſt not only be allowed for in this, but

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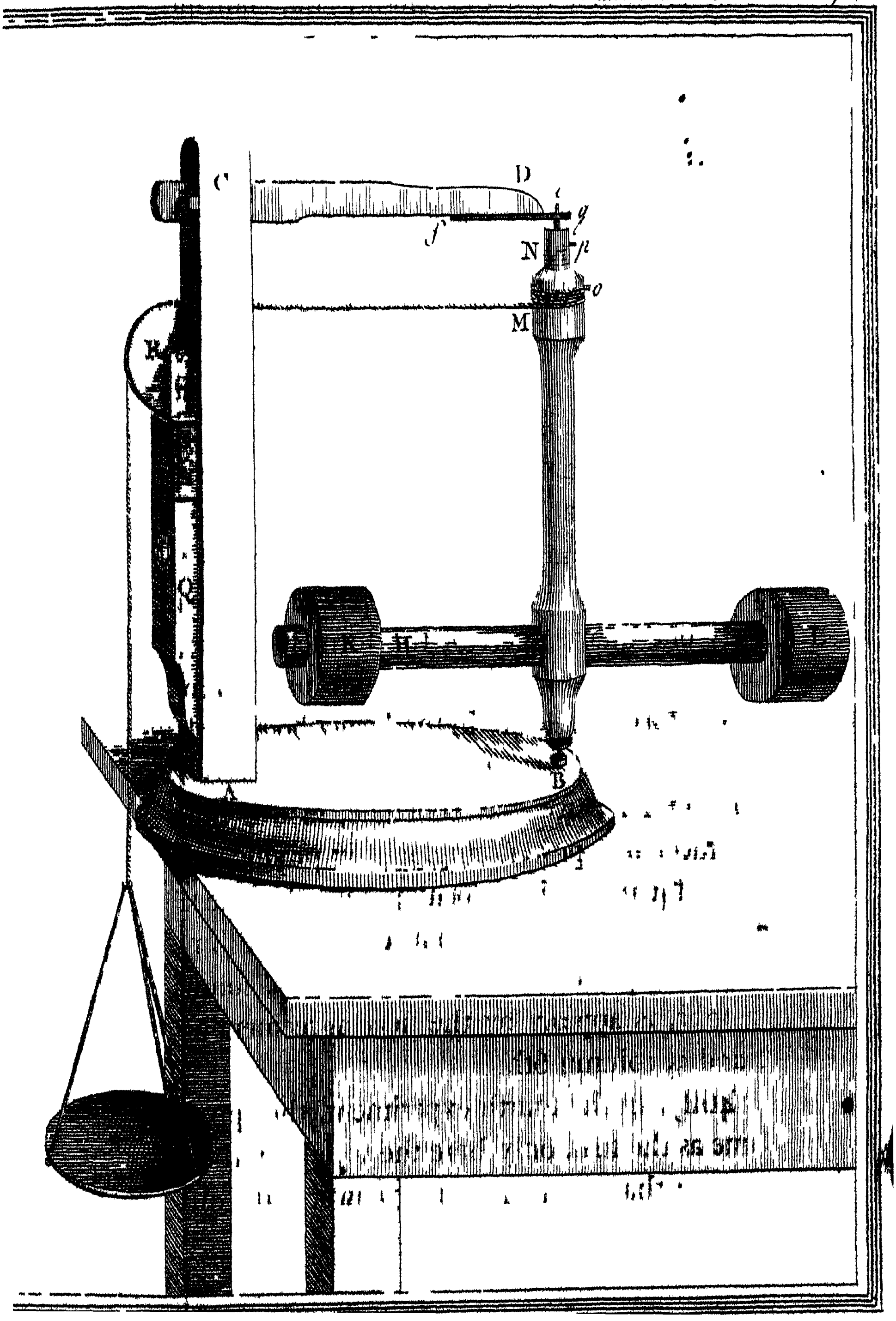
the reſt; but as the impelling power is acting here upon a lever of but half the length, and, conſequently, but half the intenſity, when referred to the bodies to be moved, it takes juſt double the time to generate the ſame velocity therein.

DEDUCTION. It appears from hence, that the ſame mechanic power is capable of producing the ſame velocity in a given body, whether it is applied ſo as to produce it in a greater or a leſſer time; but that the time taken to produce a given velocity, by an uniformly continued action, is in a ſimple inverſe proportion of the intenſity of the impulſive power.

3dly, The third experiment being made with $2\frac{1}{2}$ turns of the leſſer barrel, the ſame weight in the ſcale of 8 ounces deſcending only one quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, *viz.* $50\frac{1}{2}$; but as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in $58''\frac{1}{2}$; that is, nearly 10 revolutions in $29''$; we may conclude, in this inſtance, that the mechanic power, employed in producing motion, is as the ſquare of the velocity produced in the ſame body; and that the velocity produced is as the time that an impelling power, of the ſame intenſity, continues to act upon it, as appears by the near agreement of numbers 2 and 3, column 6th.

4thly, In the fourth experiment, the apparatus is the ſame as the firſt, only here the weight in the ſcale is 32 ounces; that is, the impelling power is the quadruple of the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the firſt, and hereby a double velocity is given to the bodies; for they make 20 revolutions in 14″, which is a ſmall matter leſs than half the time taken up in making 20 revolutions in the firſt experiment. It alſo appears, that the velocity acquired is ſimply as the impelling power compounded with the time of its action; for a quadruple impulſion acting for 7″ inſtead of 14″ generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being four times greater than the firſt, it holds here alſo, that the mechanic power, to be neceſſarily employed, is as the ſquare of the velocity to be generated; that is, in the ſame proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the firſt.

5thly, The fifth and ſixth experiments were made with a mechanic power four times greater than thoſe employed in numbers 2 and 3 reſpectively; and ſince the ſame deductions reſult from hence as from numbers 2 and 3, they are additional confirmations of the concluſions drawn from them and from the laſt article.

6thly, In the ſeventh experiment, the diſpoſition of the apparatus is the ſame as number 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the ſame mechanic power ſtill produces the ſame velocity in the ſame bodies; for though 20 revolutions were performed in $14\frac{3}{4}$″ (ſee column 7)

which is nearly half the time that 20 revolutions were performed in the firſt experiment; yet, ſince the circles in which the bodies revolved in the ſeventh are only of half the circumference as thoſe of number 1, it is obvious, that the abſolute velocity acquired by the moving bodies in the two caſes is equal. But, by column 6th, the time in which it was generated is only half; yet, notwithſtanding, this will coincide with the former concluſions, if the intenſity of the impelling power is compounded therewith; for, though the barrel was the ſame with the ſame number of turns as in number 1, and therefore the lever the ſame, by which the impelling power acted, yet, as the bodies, upon which this lever was to act, were placed upon a lever of only half the length from the center, the impelling power, acting by the firſt lever, would act upon the ſecond with double the intenſity, according to the known laws of mechanics; that is, it would require a double weight oppoſing the bodies, to prevent their moving, in order to balance it. An impulſive power, therefore, of double the intenſity, acting for half the time, produces the ſame effect in generating motion, as an impulſive power, of half the intenſity, acting for the whole time.

7thly, The eighth and ninth experiments afford the ſame deductions and confirmations relative to the ſeventh experiment, that the fifth and ſixth do reſpecting the fourth, and that the ſecond and third do reſpecting the firſt; and from the near agreement of the whole, when the

the neceſſary allowances before mentioned are made, together with ſome ſmall inequality ariſing from the mechanical power loſt by the difference of the motion given by gravity to the weight in the ſcale: I ſay, from theſe agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may ſafely conclude, that this is the univerſal law of nature, reſpecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power neceſſary to be employed to produce or generate different velocities (the bodies being ſuppoſed equal in their quantity of matter); that the mechanic powers to be expended are as the ſquares of the velocities to be generated, and *vice verſâ*; and that the ſimple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice verſâ*.

We ſhall, perhaps, form a ſtill clearer conception of the relation between velocities produced, and the quantities of mechanic power required to produce them; together with the collateral circumſtances attending, by which theſe propoſitions, ſeemingly two, are reconciled and united, by ſtating the following popular elucidation, which, indeed, was the original idea that occurred to me on conſidering this ſubject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppoſe then a large iron ball of 10 feet diameter, turned truly ſpherical, and ſet upon an extended plane of the ſame metal, and truly level. Now, if a man begins to

push at it, he will find it very resisting to motion at first; but, by continuing the impulse, he will gradually get it into motion, and having nothing to resist it but the air, he will, by continuing his efforts, at length get it to roll almost as fast as he can run. Suppose now, in the first minute he gets it rolled through a space of one yard; by this motion, proceeding from rest (similar to what happens to falling bodies) it would continue to roll forward at the rate of two yards *per* minute, without further help; but supposing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a space of two yards more, in addition to the former, that is, at the rate of four yards *per* minute; and at the end of the third minute, he has again added an equal increase of velocity, and made it proceed at the rate of six yards *per* minute; and so on, increasing its velocity at the rate of two yards in every minute. The man, therefore, in the space of every minute exerts an equal impulse upon the ball, and generates an equal increase of movement correspondent to the definition of Sir ISAAC NEWTON. But let us see what happens besides: the man, in the first minute, has moved but one yard from where he set out; but he must in the second minute move two yards more, in order to keep up with the ball; and as he exerted an impulse upon it, so as at the end of second minute to have given it an additional velocity of the two yards, he must also in this time have gradually changed its velocity from the rate of two yards *per* minute to that of four, and the space, that he will of conse-

quence have actually been obliged to go through in the ſecond minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the ſecond minute; ſo that being one yard from his original place at the beginning of the ſecond minute, at the end of it he will have moved the ſum of the journies of the firſt and ſecond minute, that is, in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards *per* minute, in the third minute he muſt travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; ſo that in the third minute, he muſt travel five yards to keep up the ſame impelling power upon the ball that he did in the firſt minute in travelling one, ſo that theſe five yards in the third minute, added to the four yards that he had travelled in the two preceding minutes, ſets him at the end of the third minute nine yards from whence he ſet out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of ſix yards *per* minute, as before ſtated. We may now leave the further purſuit of theſe proportions, and ſee how the account ſtands. He generated a velocity of two yards *per* minute in the firſt minute, the ſquare of which is four, when he had moved but one yard from his place; and he had generated a velocity of ſix yards *per* minute, the ſquare of which is thirty-ſix, at the end of the third minute, when he had travelled nine yards from his place. Now, ſince the ſquare of the velocity, generated at the end of the firſt minute, is to that

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of the velocity generated at the end of the third minute, as 4 : 36, that is, as 1 : 9; and ſince the ſpaces, moved through by the man to communicate theſe velocities, are alſo as 1 : 9, it follows, that the ſpaces through which the man muſt travel, in order to generate theſe velocities reſpectively (preſerving the impelling power perfectly equal), muſt be as the ſquares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally reſiſting, this would be the meaſure of what the man has done in order to give motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that muſt of neceſſity be employed in giving different degrees of velocity to the ſame body, muſt be as the ſquare of that velocity; and if the converſe of this propoſition did not hold, *viz.* that if a body in motion, in being ſtopped, would not produce a mechanical effect equal or proportional to the ſquare of its velocity, or to the mechanical power employed in producing it, the effect would not correſpond with its producing cauſe.

Thus the conſequences of generating motion upon a level plane exactly correſpond with the generating of motion by gravity; *viz.* that though in two ſeconds of time the equal impulſive power of gravity gives twice the velocity to a body that it does in one ſecond, yet this collateral circumſtance attends it, that at the end of the double time, in conſequence of the velocity acquired in

the firſt half, the body has fallen from where it ſet forward through four times the perpendicular; and, therefore, though the velocity is only doubled, yet four times the mechanical power has been conſumed in producing it, as four times the mechanical power muſt be expended in bringing up the fallen body to its firſt place.

This then appears to be the foundation, not only of the diſputes that have ariſen, but of the miſtakes that have been made, in the application of the different definitions of quantity of motion; that while thoſe, that have adhered to the definition of Sir ISAAC NEWTON, have complained of their adverſaries, in not conſidering the time in which effects are produced, they themſelves have not always taken into the account the ſpace that the impelling power is obliged to travel through, in producing the different degrees of velocity. It ſeems, therefore, that, without taking in the collateral circumſtances both of time and ſpace, the terms, quantity of motion, *momentum*, and force of bodies in motion, are abſolutely indefinite; and that they cannot be ſo eaſily, diſtinctly, and fundamentally compared, as by having recourſe to the common meaſure, *viz.* mechanic power.

From the whole of what has been inveſtigated, it therefore appears, that time, properly ſpeaking, has nothing to do with the production of mechanical effects, otherwiſe than as, by equally flowing, it becomes a common meaſure; ſo that, whatever mechanical effect is found to be produced in a given time, the uniform continuance of the action of the ſame mechanical power will, in a double time,

time, produce two ſuch effects, or twice that effect. A mechanical power, therefore, properly ſpeaking, is meaſured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a leſſer time; thus, having treaſured up 1000 tuns of water, which I can let out upon the over-ſhot wheel of a mill, and deſcending through a perpendicular of 20 feet, this power applied to proper mechanic inſtruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that, at a certain rate of expending it, it will grind this corn in an hour. But ſuppoſe the mill equally adapted to produce a proportionable effect, by the application of a greater impulſive power as with a leſs, then, if I let out the water twice as faſt upon the wheel, it will grind the corn twice as faſt, and both the water will be expended and the corn ground in half an hour. Here the ſame mechanical effect is produced; *viz.* the grinding a given quantity of corn, by the ſame mechanical power, *viz.* 1000 tuns of water deſcending through a given perpendicular of 20 feet, and yet this effect is in one caſe produced in half the time of the other. What time, therefore, has to do in the buſineſs is this: let the rate of doing the buſineſs, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the ſame rate, twice the effect will be produced in twice the time, on ſuppoſition that I have a ſupply of mechanic power to go on with. Thus 1000 tuns of water, deſcending through 20 feet of perpendicular,

dicular, being, as has been ſhewn, a given mechanic power, let me expend it at what rate I will, if when this is expended, I muſt wait another hour before it be renewed, by the natural flow of a river, or otherwiſe, I can then only expend twelve ſuch quantities of power in 24 hours; but if, while I am expending 1000 tuns in one hour, the ſtream renews me the ſame quantity, then I can expend 24 ſuch quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common meaſure; but the quantity of mechanic power ariſing from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.

XXVIII. *A new and general Method of finding simple and quickly-converging Series; by which the Proportion of the Diameter of a Circle to its Circumference may easily be computed to a great Number of Places of Figures. By* Charles Hutton, *Esq. F. R. S.*

TO THE REV. DR. HORSLEY, SEC. R. S.

Royal Mil. Acad.
Jan. 25, 1776.

SIR,

R. May 1, 1776.

IN a late examination of the methods of Mr. MACHIN and others, for computing the proportion of the diameter of a circle to its circumference, I discovered the method which is explained in the paper accompanying this letter. This method you, SIR, will perceive is very general, and discovers many series which are very fit for the abovementioned purpose. If you think it has sufficient merit to entitle it to the honour of being offered to the Royal Society, I have taken the liberty to inclose it to you, requesting the favour of you to present it accordingly, from, &c.

P. S. In the course of the last year, I attended some very interesting experiments on the effects of the

force

force of fired gun-powder; as they feem to lead to many important conclufions, I believe I fhall, in a fhort time, have the honour of fubmitting to your infpection an account of fome of them, for the like purpofe as the following paper.

THE excellency of this method is primarily owing to the fimplicity of the feries by which an arc is found from its tangent. For if t denote the tangent of an arc a, the radius being 1, then it is well known, that the arc a will be equal to the infinite feries,

$$\frac{t}{1} - \frac{t^3}{3} + \frac{t^5}{5} - \frac{t^7}{7} + \frac{t^9}{9} - \frac{t^{11}}{11} + \&c.$$

where the form is as fimple as can be defired. And it is evident, that nothing farther is required than to contrive matters fo, as that the value of the quantity t in this feries may be both a fmall and a very fimple number. Small, that the feries may be made to converge fufficiently faft; and fimple, that the feveral powers of t may be raifed by eafy multiplications, or eafy divifions.

Since the firft difcovery of the above feries, many have ufed it, and that after different methods, for determining the length of the circumference to a great number of figures. Among thefe were Dr. HALLEY, Mr. ABRAHAM SHARP, Mr. MACHIN, and others, of our own country; and M. DE LAGNY, M. EULER, &c. abroad. Dr. HALLEY ufed the arc of 30°, or $\frac{1}{12}$th of the circumference, the tangent of which being $=\sqrt{\frac{1}{3}}$, by fubftituting $\sqrt{\frac{1}{3}}$ for t in the above feries, and multiplying by 6, the femi

circumference is $=6\sqrt{\tfrac{1}{3}}\times: 1-\frac{1}{3\cdot3}+\frac{1}{5\cdot3^2}-\frac{1}{7\cdot3^3}+\frac{1}{9\cdot3^4}$, &c. which series is, to be sure, very simple; but its rate of converging is not very great, on which account a great many terms must be used to compute the circumference to many places of figures. By this very series, however, the industrious Mr. SHARP computed the circumference to 72 places of figures; Mr. MACHIN extended it to 100; and M. DE LAGNEY, still by the same series, continued it to 128 places of figures. But although this series, from the 12th part of the circumference, does not converge very quickly, it is, perhaps, the best aliquot part of the circumference which can be used for this purpose; for when smaller arcs, which are exact aliquot parts, are used, their tangents, although smaller, are so much more complex, as to render them, on the whole, more operose in the application; this will easily appear, by inspecting some instances, that have been given by Mr. GARDINER, in his editions of SHERWIN's Tables. One of these methods is from the arc of 18°, the tangent of which is $\sqrt{1-2\sqrt{\tfrac{1}{5}}}$; another is from the arc of $22°\tfrac{1}{2}$, the tangent of which is $\sqrt{2}-1$; and a third is from the arc of 15°, the tangent of which is $2-\sqrt{3}$. All of which are evidently too complex to afford an easy application to the general series.

In order to a still farther improvement of the method by the above general series, Mr. MACHIN, by a very singular and excellent contrivance, has greatly reduced the

labour

labour naturally attending it. His method is explained in Mr. MASERES's Appendix to his Dissertation on the Use of the Negative Sign in Algebra; and I have given an analysis of it, or a conjecture concerning the manner in which it is probable Mr. MACHIN discovered it, in my Treatise on Mensuration; which, I believe, are the only two books in which that method has been explained, as I never had seen it explained by any, till I met with Mr. MASERES's book abovementioned on the Use of the Negative Sign. For though the series' discovered by that method were published by Mr. JONES, in his *Synopsis Palmariorum Matheseos*, which was printed in the year 1706, he has given them merely by themselves, without the least hint of the manner in which they were obtained. The result shews, that the proportion of the diameter to the circumference is equal to that of 1 to quadruple the sum of the two series',

$$\begin{cases} \frac{4}{5} \times : 1 - \frac{1}{3.5^2} + \frac{1}{5.5^4} - \frac{1}{7.5^6} + \frac{1}{9.5^8}, \text{ \&c.} \\ \frac{1}{239} \times : 1 - \frac{1}{3.239^2} + \frac{1}{5.239^4} - \frac{1}{7.239^6} + \frac{1}{9.239^8}, \text{ \&c.} \end{cases}$$

The slower of which converges almost thrice as fast as Dr. HALLEY's raised from the tangent of 30°. The latter of these two series converges still a great deal quicker; but then the large incomposite number 239, by the reciprocals of the powers of which the series converges, occasions such long, tedious divisions, as to counter-balance its quickness of convergency; so that the former series is summed, with rather more ease than the latter, to the same number

ber of places of figures. Mr. JONES, in his *Synopsis*, mentions other ſeries' beſides this, which he had received from Mr. MACHIN for the ſame purpoſe, and drawn from the ſame principle. But we may conclude this to be the beſt of them all, as he did not publiſh any other beſides it.

M. EULER too, in his *Introductio in Analyſin Infinitorum*, by a contrivance ſomething like Mr. MACHIN's, diſcovers, that $\frac{1}{2}$ and $\frac{1}{3}$ are the tangents of two arcs, the ſum of which is juſt 45°; and that, therefore, the diameter is to the circumference as 1 to quadruple the ſum of the two ſeries',

$$\begin{cases} \frac{1}{2} \times : 1 - \frac{1}{3 \cdot 4} + \frac{1}{5 \cdot 4^2} - \frac{1}{7 \cdot 4^3} + \frac{1}{9 \cdot 4^4}, \text{ \&c.} \\ \frac{1}{3} \times : 1 - \frac{1}{3 \cdot 9} + \frac{1}{5 \cdot 9^2} - \frac{1}{7 \cdot 9^3} + \frac{1}{9 \cdot 9^4}, \text{ \&c.} \end{cases}$$

Both which ſeries' converge much faſter than Dr. HALLEY's, and are yet at the ſame time made to converge by the powers of numbers producing only ſhort diviſions; that is, diviſions performed in one line, or without writing down any thing beſides the quotients.

I come now to explain my own method, which, indeed, bears ſome little reſemblance to the methods of MACHIN and EULER; but then it is more general, and diſcovers, as particular caſes of it, both the ſeries' of thoſe gentlemen and many others, ſome of which are fitter for this purpoſe than theirs are.

4. This method then conſiſts in finding out ſuch ſmall arcs, as have for tangents ſome ſmall and ſimple vulgar fractions (the radius being denoted by 1), and ſuch alſo that

ſome

ſome multiple of thoſe arcs ſhall differ from an arc of 45°, the tangent of which is equal to the radius, by other ſmall arcs, which alſo ſhall have tangents denoted by other ſuch ſmall and ſimple vulgar fractions. For it is evident, that if ſuch a ſmall arc can be found, ſome multiple of which has ſuch a propoſed difference from an arc of 45°, then the lengths of theſe two ſmall arcs will be eaſily computed from the general ſeries, becauſe of the ſmallneſs and ſimplicity of their tangents; after which, if the proper multiple of the firſt arc be increaſed or diminiſhed by the other arc, the reſult will be the length of an arc of 45°, or $\frac{1}{8}$th of the circumference. And the manner in which I diſcover ſuch arcs is thus:

Let T, *t*, denote any two tangents, of which T is the greater, and *t* the leſs; then it is known, that the tangent of the difference of the correſponding arcs is equal to $\frac{T-t}{1+Tt}$. Hence, if *t*, the tangent of the ſmaller arc, be ſucceſſively denoted by each of the ſimple fractions $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. the general expreſſion for the tangent of the difference between the arcs will become reſpectively $\frac{2T-1}{2+T}$, $\frac{3T-1}{3+T}$, $\frac{4T-1}{4+T}$, $\frac{5T-1}{5+T}$, &c.; ſo that if T be expounded by any given number, then theſe expreſſions will give the tangent of the difference of the arcs in known numbers, according to the values of *t*, ſeverally aſſumed reſpectively. And if, in the firſt place, T be equal to 1, the tangent of 45°, the foregoing expreſſions will give the tangent of an arc, which is equal to the difference between that of 45° and the firſt arc, or that of which the

 tangent

tangent is one of the numbers $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. Then if the tangent of this difference, juſt now found, be taken for T, the ſame expreſſions will give the tangent of an arc, which is equal to the difference between the arc of 45° and the double of the firſt arc. Again, if for T we take the tangent of this laſt found difference, then the foregoing expreſſions will give the tangent of an arc, equal to the difference between that of 45° and the triple of the firſt arc. And again taking this laſt found tangent for T, the ſame theorem will produce the tangent of an arc equal to the difference between that of 45° and the quadruple of the firſt arc; and ſo on, always taking for T the tangent laſt found, the ſame expreſſions will give the tangent of the difference between the arc of 45° and the next greater multiple of the firſt arc; or that, of which the tangent was at firſt aſſumed equal to one of the ſmall numbers $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. This operation, being continued till ſome of the expreſſions give ſuch a fit, ſmall, and ſimple fraction as is required, is then at an end, for we have then found two ſuch ſmall tangents as were required, *viz.* the tangent laſt found, and the tangent firſt aſſumed.

Here follow the ſeveral operations adapted to the ſeveral values of t. The letters a, b, c, d, &c. denote the ſeveral ſucceſſive tangents.

1. Take $t = \frac{1}{2}$, then the theorem $\frac{2T-1}{2+T}$ gives

$$a = \frac{1}{3}$$

$$b = \frac{-1}{7}$$

Therefore the arc of 45°, or $\frac{1}{8}$th of the circumference, is either

either equal to the sum of the two arcs of which $\frac{1}{2}$ and $\frac{1}{3}$ are the tangents, or to the difference between the arc of which the tangent is $\frac{1}{7}$, and the double of the arc of which the tangent is $\frac{1}{2}$; that is, putting A = the arc of 45°.

$$A=\begin{cases} +\frac{1}{2}\times : 1-\frac{1}{3\cdot 4}+\frac{1}{5\cdot 4^2}-\frac{1}{7\cdot 4^3}+\frac{1}{9\cdot 4^4}-\&c. \\ +\frac{1}{3}\times : 1-\frac{1}{3\cdot 9}+\frac{1}{5\cdot 9^2}-\frac{1}{7\cdot 9^3}+\frac{1}{9\cdot 9^4}-\&c. \end{cases}$$

$$\text{Or, } A=\begin{cases} +1-\frac{1}{3\cdot 4}+\frac{1}{5\cdot 4^2}-\frac{1}{7\cdot 4^3}+\frac{1}{9\cdot 4^4}-\&c. \\ -\frac{1}{7}\times : 1-\frac{1}{3\cdot 49}+\frac{1}{5\cdot 49^2}-\frac{1}{7\cdot 49^3}+\frac{1}{9\cdot 49^4}-\&c. \end{cases}$$

And the former of these values of A is the same with that before mentioned as given by M. EULER; but the latter is much better, as the powers of $\frac{1}{49}$ converge much faster than those of $\frac{1}{9}$.

COROL. From double the former of these values of A subtracting the latter, the remainder is,

$$A=\begin{cases} +\frac{2}{3}\times : 1-\frac{1}{3\cdot 9}+\frac{1}{5\cdot 9^2}-\frac{1}{7\cdot 9^3}+\frac{1}{9\cdot 9^4}-\&c. \\ +\frac{1}{7}\times : 1-\frac{1}{3\cdot 49}+\frac{1}{5\cdot 49^2}-\frac{1}{7\cdot 49^3}+\frac{1}{9\cdot 49^4}-\&c. \end{cases}$$

which is a much better theorem than either of the former.

2. If t be taken $=\frac{1}{3}$, then the expression $\frac{3T-1}{3+T}$ gives,

$$b=\frac{1}{7}$$

Here the value of $a=\frac{1}{2}$ gives the same expression for the value of A as the first in the foregoing case, and the value

 of

of $b=\frac{1}{7}$ gives the value of A the very same as in the corollary to the case above.

3. Taking $t=\frac{1}{4}$, the expression $\frac{4\text{T}-1}{4+\text{T}}$ gives

$$a = \frac{3}{5}$$

$$b = \frac{7}{23}$$

$$c = \frac{5}{99}$$

$$d = -\frac{79}{401}$$

Here it is evident, that the value of $c=\frac{5}{99}$ is the fittest number afforded by this case; and from it it appears, that the arc of 45° is equal to the sum of the arc of which the tangent is $\frac{5}{99}$, and the triple of the arc of which the tangent is $\frac{1}{4}$.

$$\text{Or that A}=\begin{cases} +\frac{3}{4}\times : 1 - \frac{1}{3.16} + \frac{1}{5.16^2} - \frac{1}{7.16^3} + \frac{1}{9.16^4} - \text{\&c.} \\ +\frac{5}{99}\times : 1 - \frac{5^2}{3.99^2} + \frac{5^4}{5.99^4} - \frac{5^6}{7.99^6} + \frac{5^8}{9.99^8} - \text{\&c.} \end{cases}$$

Which is the best theorem that we have yet found, because that the number 99 resolves into the two easy factors 9 and 11.

4. Let now t be taken $=\frac{1}{5}$, and the expression $\frac{5\text{T}-1}{5+\text{T}}$ will give

2

$$b = \frac{7}{17}$$

$$c = \frac{9}{46}$$

$$d = \frac{-1}{239}$$

Where

Where it is evident, that the laſt number, or the value of d, is the fitteſt of thoſe produced in this caſe, and from which it appears, that the arc of 45° is equal to the difference between the arc of which the tangent is $\frac{1}{239}$, and quadruple the arc of which the tangent is $\frac{1}{5}$. Or that

$$A = \begin{cases} +\frac{4}{5} \times : 1 - \frac{1}{3.5^2} + \frac{1}{5.5^4} - \frac{1}{7.5^6} + \frac{1}{9.5^8} - \&c. \\ \cdot\frac{1}{239} \times : 1 - \frac{1}{3.239^2} + \frac{1}{5.239^4} - \frac{1}{7.239^6} + \frac{1}{9.239^8} - \&c. \end{cases}$$

Which is the very theorem that was invented by Mr. MACHIN, as we have before mentioned.

5. Take now $t = \frac{1}{6}$, and the expreſſion $\frac{6T-1}{6+T}$ will give

$$a = \frac{5}{7}$$
$$b = \frac{23}{47}$$
$$c = \frac{91}{305}$$
$$d = \frac{241}{1921}$$
$$e = \frac{-475}{11767}$$

Of which numbers none, it is evident, are fit for our purpoſe.

 6. Again,

6. Again, take $t = \frac{1}{7}$, and the expression $\frac{7T - 1}{7 + T}$ will give

$$a = \frac{3}{4}$$

17
$-$
31
11

$$d = \frac{49}{205}$$
$$e = \frac{69}{742}$$
$$f = \frac{-259}{5263}$$

Neither are any of these fit numbers for our purpose.

7. In like manner take $t = \frac{1}{8}$, so shall $\frac{8T - 1}{8 + T}$ give

$$a = \frac{7}{9}$$
$$b = \frac{47}{79}$$
$$c = \frac{297}{679}$$
$$d = \frac{1697}{5729}$$
$$e = \frac{7847}{47529}$$
$$f = \frac{14047}{388079}$$

8. And

. And if t be taken $=\frac{1}{9}$, the expreſſion $\frac{9t-1}{9+t}$ will give

$$a = \frac{4}{5}$$

$$\frac{31}{49}$$

$$c = \frac{115}{236}$$

$$d = \frac{799}{2239}$$

$$e = \frac{2467}{10475}$$

$$f = \frac{11809}{96751}$$

$$g = \frac{4765}{441284}$$

. Alſo, if we take $t=\frac{1}{10}$, the expreſſion $\frac{10t-1}{10+t}$ will give.

$$a = \frac{9}{11}$$

$$b = \frac{79}{119}$$

$$c = \frac{671}{1269}$$

$$d = \frac{5441}{13361}$$

$$e = \frac{41049}{139051}$$

$$f = \frac{271439}{1431559}$$

$$g = \frac{1282831}{14587029}$$

10. Farther

10. Farther, if we take $t=\frac{1}{11}$, the expression $\frac{11T-1}{11+T}$ will give

$$a = \frac{5}{6}$$

$$b = \frac{49}{71}$$

$$c = \frac{234}{415}$$

$$d = \frac{2159}{4799}$$

$$e = \frac{9475}{27474}$$

$$f = \frac{76751}{311689}$$

$$g = \frac{266286}{1752665}$$

$$h = \frac{1176481}{19545601}$$

11. Lastly, if we take $t=\frac{1}{12}$, the expression $\frac{12T-1}{12+T}$ gives

$$a = \frac{11}{13}$$

$$b = \frac{113}{167}$$

$$c = \frac{41}{73}$$

$$d = \frac{419}{917}$$

$$e = \frac{4111}{11423}$$

$$f = \frac{37909}{141187}$$

$$g = \frac{313721}{1732153}$$

$$h = \frac{2032499}{21099557}$$

$$i = \frac{3290431}{255227183}$$

Here

Here it is evident, that none of theſe latter caſes afford any numbers that are fit for this purpoſe. And to try any other fractions leſs than $\frac{1}{12}$ for the value of t, does not ſeem likely to anſwer any good purpoſe, eſpecially, as the diviſors, after 12, become too large to be managed in the eaſy way of ſhort diviſion in one line.

By the foregoing means it appears, that we have diſcovered five different forms of the value of A, or $\frac{1}{4}$th of the ſemi-circumference, all of which are very proper for readily computing its length; *viz.* three forms in the firſt caſe and its corollary, one in the third caſe, and one in the fourth caſe. Of theſe, the firſt and laſt are the ſame as thoſe invented by EULER and MACHIN reſpectively, and the other three are quite new, as far as I know.

But another remarkable excellency attending the firſt three of the before mentioned ſeries is, that they are capable of being changed into others which not only converge ſtill faſter, but in which the converging quantity ſhall be $\frac{1}{10}$, or ſome multiple or ſub-multiple of it, and ſo the powers of it raiſed with the utmoſt eaſe. The ſeries', or theorems, here meant are theſe three:

$$1\text{ſt, A}=\begin{cases} +\frac{1}{2}\times \qquad \frac{1}{3\cdot4}+\frac{1}{5\cdot4^2}-\frac{1}{7\cdot4^3}+\frac{1}{9\cdot4^4}, \text{ \&c.} \\ +\frac{1}{3}\times : 1-\frac{1}{3\cdot9}+\frac{1}{5\cdot9^2}-\frac{1}{7\cdot9^3}+\frac{1}{9\cdot9^4}, \text{ \&c.} \end{cases}$$

$$2\text{dly, A}=\begin{cases} +1-\frac{1}{3\cdot4}+\frac{1}{5\cdot4^2}-\frac{1}{7\cdot4^3}+\frac{1}{9\cdot4^4}, \text{ \&c.} \\ -\frac{1}{7}\times : 1-\frac{1}{3\cdot49}+\frac{1}{5\cdot49^2}-\frac{1}{7\cdot49^3}+\frac{1}{9\cdot49^4}, \text{ \&c.} \end{cases}$$

3dly,

$$3\text{dly, } A = \begin{cases} +\frac{2}{3} \times : 1 - \frac{1}{3\cdot 9} + \frac{1}{5\cdot 9} - \frac{1}{7\cdot 9^3} + \frac{1}{9\cdot 9^4}, \text{ \&c.} \\ +\frac{1}{7} \times : 1 - \frac{1}{3\cdot 49} + \frac{1}{5\cdot 49^2} - \frac{1}{7\cdot 19^3} + \frac{1}{9\cdot 49}, \text{ \&c.} \end{cases}$$

Now if each of these be transformed, by means of the differential series in cor. 3. p. 64. of the late Mr. THOMAS SIMPSON's Mathematical Dissertations, they will become of these very commodious forms, *viz.*

$$1\text{st, } A = \begin{cases} +\frac{4}{10} \times : 1 + \frac{4}{3\cdot 10} + \frac{8\alpha}{5\cdot 10} + \frac{12\beta}{7\cdot 10} + \frac{16\gamma}{9\cdot 10}, \text{ \&c.} \\ +\frac{3}{10} \times : 1 + \frac{2}{3\cdot 10} + \frac{4\alpha}{5\cdot 10} + \frac{6\beta}{7\cdot 10} + \frac{8\gamma}{9\cdot 10}, \text{ \&c.} \end{cases}$$

$$2\text{dly, } A = \begin{cases} +\frac{4}{5} \times : 1 + \frac{4}{3\cdot 10} + \frac{8\alpha}{5\cdot 10} + \frac{12\beta}{7\cdot 10} + \frac{16\gamma}{9\cdot 10}, \text{ \&c.} \\ -\frac{7}{50} \times : 1 + \frac{4}{3\cdot 100} + \frac{8\alpha}{5\cdot 100} + \frac{12\beta}{7\cdot 100} + \frac{16\gamma}{9\cdot 100}, \text{ \&c.} \end{cases}$$

$$3\text{dly, } A = \begin{cases} +\frac{6}{10} \times : 1 + \frac{2}{3\cdot 10} + \frac{4\alpha}{5\cdot 10} + \frac{6\beta}{7\cdot 10} + \frac{8\gamma}{9\cdot 10}, \text{ \&c.} \\ +\frac{7}{50} \times : 1 + \frac{2}{3\cdot 50} + \frac{4\alpha}{5\cdot 50} + \frac{6\beta}{7\cdot 50} + \frac{8\gamma}{9\cdot 50}, \text{ \&c.} \end{cases}$$

Where α, β, γ, &c. denote always the preceding terms in each series.

Now it is evident, that all these latter series' are much easier than the former ones, to which they respectively correspond; for, because of the powers of 10 here concerned, we have little more to do than to divide by the series of odd numbers 1, 3, 5, 7, 9, &c.

Of all these three forms the second is the fittest for computing the required proportion; because that, of the two series' of which it consists, the several terms of the one

6 are

are found from the like terms of the other, by dividing theſe latter by 10 and its ſeveral ſucceſſive powers 100, 1000, &c.; that is, the terms of the one conſiſt of the ſame figures as the terms of the other, only removed a certain number of places farther towards the right, in the decuple ſcale of numbers; and the number of places, by which they muſt be removed, is the ſame as the diſtance of each term from the firſt term of the ſeries, *viz.* in the ſecond term the figures muſt be moved one place lower, in the third term two, in the fourth term three, &c. ſo that the latter ſeries will conſiſt of but about half the number of the terms of the former. Thus, then, this method may be ſaid to effect the buſineſs by one ſeries only, in which there is little more to do, than to divide by the ſeveral numbers 1, 3, 5, 7, &c.; for as to the multiplications by the numbers in the numerators of the terms, after they become large, they are eaſily performed by barely multiplying by the number two, and ſubtracting one number from another: for ſince every numerator is leſs by two than the double of its denominator, if d denote any denominator (excluſive always of the powers of 10) then the co-efficient of that term is $\frac{2d-2}{d}$, or $2-\frac{2}{d}$, by which the preceding term is to be multiplied; to do which, therefore, multiply it by two, that is double it, and divide that double by the diviſor d, and ſubtract the quotient from the ſaid double.

By a pretty exact estimate, which I have made, of the proportion of the trouble or time in computing the circumference by this middle form of the value of A, and by Mr. MACHIN's theorem, I have found, that the computation by his method requires about $\frac{1}{8}$th or $\frac{1}{10}$th more time than by mine. And its advantage over any of the series' invented by EULER or others, is still much more considerable.

XXIX. *An*

XXIX. *An Account of a very extraordinary Effect of Lightning on a Bullock, at* Swanborow, *in the Parish of* Iford *near* Lewes, *in* Suffex. *In fundry Letters, from Mr.* James Lambert, *Landfcape-Painter at* Lewes; *and One from* William Green, *Efquire, at* Lewes, *to* William Henly, *F. R. S.*

LETTER I.

FROM MR. LAMBERT.

Sept. 13, 1774.

R. May 1, 1776.

I SHALL now inform you of a very extraordinary and fingular effect of lightning on a bullock in this neighbourhood, which happened about a fortnight fince. The bullock is pyed, white and red. The lightning, as fuppofed, ftripped off all the white hair from his back, but left the red hair without the leaft injury. I have been to fee the bullock, and have made a drawing of it, which I will fend you as foon as I can get more particulars from Mr. Rogers, the proprietor; for, when I faw him, I omitted to afk him, if the hair was all off (as it now appears) when it was firft feen the next morning; and whether any hair was found in the field; and if it appeared to be finged or not? The

 bullock

bullock does not ſeem to have been hurt; his ſkin looks fair and well. Mr. ROGERS informs me, that he has had two other bullocks ſtruck in the ſame manner; one laſt ſummer, that was all white, was ſtripped of his hair like this, though not all over his back, but chiefly on his ſhoulders; the other, two years ſince, was pyed, and affected much like the preſent. He thinks, it cannot be the effect of any diſeaſe, becauſe the beaſts were all in good health before and after this accident happened. He is more inclined to think it was the effect of lightning, becauſe when he has had cattle diſordered, ſo as to make their hair come off, he has never obſerved white hair to come off more than red, &c.; but that it has, if party-coloured, fallen off promiſcuouſly, and generally in patches; and alſo by ſlow degrees, and never ſuddenly, as in the caſe of theſe bullocks. I ſhall be glad to know your thoughts on this matter, whether it is a new circumſtance to you or not; and if you think it much worth attending to. I am, &c.

LETTER II.

FROM WILLIAM GREEN, ESQ.

SIR, Sept. 28, 1774.

THE incloſed account of the effect of lightning ſeems to me very extraordinary; perhaps, ſuch inſtances may be known to you: however, to be certain whether they

they are ſo or not, I have troubled you with a deſcription of this caſe; if it ſhould prove to be no novelty, you will only have the trouble of reading it.

In the evening of Sunday the twenty-eighth of Auguſt, at this place, there was an appearance of a thunder ſtorm, but we heard no report. A gentleman, who was riding near the marſhes not far diſtant from this town, ſaw two ſtrong flaſhes of lightning, ſeemingly running along the ground of the marſh, at about nine of the clock in the evening. On Monday morning, when the ſervants of Mr. ROGERS, a farmer at Swanborough, in the pariſh of Iford, went into the marſh, to fetch the oxen to their work, they found one of them, a four-year-old ſteer, ſtanding up, to appearance much burnt, and ſo weak as to be ſcarce able to walk. This was mentioned to me about a week after the accident happened; and by the deſcription of it, it ſeemed to have been ſtruck with lightning in a very uncommon manner. The ox is of a red and white colour; the white in large marks, beginning at the rump-bone, and running, in various directions, along both the ſides; the belly is all white, and the whole head and horns are white likewiſe. The lightning, with which it muſt have been undoubtedly ſtruck, fell on the rump-bone, which is white, and diſtributed itſelf along the ſides, in ſuch a manner, as to take off all the white hair from the white marks as low as the bottom of the ribs, but ſo as to leave a liſt of white hair, about half an inch broad, all round where it joined to the red; and not a ſingle hair of the red, that I can perceive, is touched.

touched. The whole belly is unhurt, but the end of the ſheath of the *penis* has the hair taken off; it is alſo taken off from the *deulap*; the horns and the curled hair on the forehead are uninjured, but it is taken off the ſides of the face, from the flat part of the jaw-bones, and it is taken off from the front of the face in ſtripes. There are a few white marks on the ſide and neck, which are ſurrounded with red, and the hair is taken off from them, leaving half an inch of white adjoining to the red. I looked attentively at the feet and legs, and could not diſcover any hair taken from them (they were very dirty) except from the joint a little above one of the hoofs, where it was partly off. I have ſent a ſketch of one of the ſides of the ox, which will ſerve to illuſtrate what I have ſaid, and is better than any deſcription. I have coloured, with faint red, thoſe parts which were ſtripped of the hair. The farmer anointed the ox with oil for a fortnight; the animal purged very much at firſt, and is greatly reduced in fleſh. I ſaw it about a fortnight ago, and it was then recovering. I am, &c.

LETTER III.

FROM MR. LAMBERT.

Nov. 15, 1774.

I AM ſorry for the delay in ſending you this drawing; but, as you know the cauſe, I need not ſay any thing further than that I have the happineſs to acquaint you, that

that I am now quite recovered from my illnefs. I have not had an opportunity of feeing Mr. GREEN, therefore cannot tell how his account may agree with mine; but I have endeavoured, as much as poffible, to get an exact ftate of the matter, having carefully infpected the bullock twice, accompanied by my nephew and a gentleman at my houfe. The creature being, as I obferved to you before, remarkably gentle, we could examine every part of him very minutely. You will fee by the drawing, that the white hair was all ftripped off from his back and down the fides, as low as the greateft diameter of his body, alfo from the top of the nofe, the upper part of both cheeks, and over the eyes, leaving the fkin quite bare; but below thofe places, under the belly, gullet, the under parts of the cheeks, the legs, and ring of white in the tail, together with an edge of white at the parting of the red and white hair, all remained without the leaft injury. We were the more particular in examining the legs, on account of your mentioning that Mr. GREEN had obferved traces of the ftroke down them to the ground, in which, I think, he muft be miftaken; for, both the times when we faw the bullock, his legs were quite clean down to the hoofs, and the hair feemed to be in a perfect ftate. If the legs had been at all affected, I think, it could not have efcaped our notice in two examinations; and there being no marks of lightning on them, inclines me to think, that the bullock was lying down at the time, and if fo, you will readily account for the under parts not being touched. The lightning being conducted by the white

hair,

hair, from the top of the back down the fides, came to the ground, at the place where the white hair is left entire; but there is one remarkable circumftance, *viz.* though all the white hair on the upper parts was taken away, as beforementioned, yet the tuft of white hair on the forehead never received any hurt at all. I have converfed with feveral farmers, &c. in hopes of getting fome information relative to thofe matters, but can meet with nothing perfectly fatisfactory. The beft account I have been able to obtain is from my neighbour Mr. TOOTH, a farrier and bullock-leach. He tells me, that this circumftance is not new to him; that he has feen a great many pyed bullocks ftruck by lightning in the fame manner as this, both in his father's time (his father being of the fame trade) and fince; and that the texture of the fkin under the white hair was always deftroyed, though looking fair at firft; and, after a while, it became fore, throwing out putrid matter in puftules, like the fmall pox with us, which in time falls off, when the hair grows again as before; and that the bullocks receive no further injury. In this ftate I found Mr. ROGERS's bullock, the fecond time I faw it, which was about a month after the firft vifit; fome of the fcabs were then dropping off, and the hair was coming on afrefh. I afked Mr. TOOTH, whether he could recollect among thofe bullocks which he, or his father, had feen ftruck dead by lightning, any that were white or pyed? But in this he could not fatisfy me; if he could, I think, it would have thrown fome light on the fubject. I

2 remember

remember perfectly well, that all the cattle that I have ſeen, which were killed by lightning, were either black, brown, or red, without any white at all in them. I muſt obſerve to you, that this bullock is both marked and affected by the ſtroke exactly alike on both ſides.

I am, &c.

HAVING been favoured with theſe letters, by gentlemen of the ſtricteſt veracity, and likewiſe particularly curious in their enquiries, I have not the leaſt reaſon to entertain a doubt of the facts they communicate; and as they may, perhaps, be productive of ſome important diſcoveries, reſpecting the different colours of bodies as conductors of electricity, I imagined, that it would not be improper to lay them before the members of the Royal Society.

To the preceding paper I would beg leave to add the following queries:

1ſt, Are not the dark-coloured hairs ſtronger in their texture than the white or light-coloured ones(a)?

2dly, If the dark-coloured hairs are the ſtrongeſt, may not this be owing to their being more deeply rooted, and partaking more largely of that nutritive matter which produces and ſupports hair? And does not the change of dark-coloured hair to grey, in perſons advanced in years, ſeem to favour this ſuppoſition?

(a) This is a fact ſo well known to houſe-painters, that they do not admit a dark hair into their bruſhes, as they would occaſion a diſagreeable roughneſs in their work. J. COVENTRY.

3dly, If the above ſuppoſitions are allowed, may not any internal injury to the ſkin (as a violent electric exploſion paſſing through it) prove more fatal to the white or light hair than to the black, red, or darker colours?

Should the above queries be all acknowledged, by thoſe gentlemen who have conſidered the ſubject, to be truths; yet, I believe, they will ſcarcely be thought ſufficient to account for the whole effect of lightning which has appeared in this caſe, and particularly for the edge of white hair adjoining to the red, which remained unhurt by it; but as they may, perhaps, in ſome meaſure have contributed towards this phenomenon, I have barely mentioned them as ſuppoſitions, and ſuppoſitions only, which have occurred to me.

A SUBSEQUENT LETTER FROM MR. LAMBERT.

DEAR SIR, Dec. 6, 1774.

I HAVE, according to your deſire, enquired of Mr. TOOTH, whether he ever ſaw a ſtroke of lightning actually fall upon a pyed bullock, ſo as to deſtroy the white hair, and ſhew the evident marks of burning, leaving the red hair uninjured? He ſays, he never did; nor can he recollect any one that has. But he gives me an account of a pyed horſe of his being killed, four or five years ſince, in a ſtable adjoining to his houſe, by a ſtroke of lightning which happened in the night; and being very great, Mr. TOOTH thinking it ſtruck his houſe, immediately

immediately got up and went to the ſtable, when he ſaw his horſe was ſtruck, and almoſt dead to appearance, though it kept on its legs near half an hour before it expired. The horſe was pyed white on the ſhoulder and moſt part of the head; that is, all the forehead and noſe, where the greateſt force of the ſtroke came. The hair was not burnt nor diſcoloured, only ſo looſened at the root, that it came off at the leaſt touch. And this is the caſe, Mr. TOOTH obſerves, with all he has ſeen or heard of, *viz.* the hair is never burnt, but the ſkin always affected, as I deſcribed it in my former letters. In the above horſe, Mr. TOOTH ſays, all the blood in the veins, under the white parts of the head, was quite ſtagnated, though he could perceive it to flow as uſual in other parts of the body, under the brown hair; and the ſkin, together with one ſide of the tongue, was parched and dried up to a greater degree than any he had ever ſeen before. The horſe ſtood in a ſtall cloſe to the door of the ſtable, which was boarded on that ſide, and through them, he thinks, the lightning ſtruck him. I am, &c.

Extract of another Letter from Mr. LAMBERT, *dated* Oct. 10, 1775, *with a Drawing* N° 3.

I HERE ſend you another inſtance of the effect of lightning on a bullock of Mr. ALSE's, at Glynd, which happened on the 20th of laſt month; it is ſimilar to the other I ſent you in every reſpect, except that I think the ſtroke on this muſt have been greater, as the ſcarf-ſkin

 ſeems

ſeems to be peeling off with the hair all over the rump, like the piece I have herewith ſent, which came off from the hip. I think too, that this is more curious, as all the red ſpots, even thoſe ſmall ones on the ſide, remain firm and ſmooth, without the leaſt injury. You will obſerve alſo, that, as in the former inſtance, after the lightning had paſſed the greateſt diameter of the body, the white hair is left intire, particularly under the belly, on the legs, &c. Mr. ALSE, having never ſeen nor heard of this wonderful phenomenon, could not conceive what was the matter with the bullock, till he ſent for Mr. TOOTH, who immediately told him the cauſe. I am, &c.

HAVING mentioned the foregoing particulars to my learned and ingenious friend Dr. A. FOTHERGILL, at Northampton, he has favoured me with ſome conjectures, which I ſhall take the liberty of annexing to this paper; *viz.* "The recent fact you mention, of the effects "of lightning on the white hair of a bullock, is ex- "tremely curious, but ſeems difficult of ſolution. Whe- "ther it can be explained from the difference of texture "between red hair and white, is doubtful; or whether "there is not ſomething peculiar in colours, as being "conductors or non-conductors of electricity, may de- "ſerve enquiry. The phlogiſton, or inflammable prin- "ciple, is thought to be the foundation of colour in bo- "dies, and to abound in proportion to the intenſity of "the colour. But phlogiſton and the electric fluid are

"probably

" probably the ſame, or at leaſt modifications of the ſame " principle; therefore, red bodies are perhaps replete " with electric matter, while white bodies may be deſti- " tute of it (b). A body ſaturated with it cannot receive " more, and may eſcape, while a neighbouring body, not " calculated to receive it, may, on its admiſſion, be de- " ſtroyed (c). Or there may exiſt a chemical affinity be- " tween electricity and the different rays of light, which, " in attracting ſome, and repelling others, may be the " foundation of many curious phenomena. But, while " we admire the effects; the habitudes and *modus ope-* " *randi* of theſe ſubtile fluids may, perhaps, for ever " elude the cognizance of our ſenſes."

(b) Many ſubſtances muſt certainly be excepted from this rule. W. HENLY.

(c) This effect of lightning generally happens to ſuch bodies which, in ſome meaſure, reſiſt its entrance, &c. merely on account of their being imperfect conductors. W. HENLY.

XX. Of

XXX. *Of the Light produced by Inflammation.* *By* George Fordyce, *M. D. F. R. S.*

R. May 9, 1776.

WHEN a body is heated to a certain degree, it becomes luminous, and is ſaid to be ignited. One of the means of producing heat is inflammation; and this, as is well known, is ſufficient for ignition. But, I apprehend, that, beſides the light produced by ignition, there is alſo light produced by the inflammation itſelf. For the inveſtigation of this principle, it will be neceſſary to conſider ignition in the firſt place.

Subſtances, heated to between 6 and 700° of FAHRENHEIT's thermometer, begin to be luminous in the dark. If they be colourleſs, the light which is firſt obſerved is red; as the heat is increaſed, there is a mixture of yellow rays; and, laſtly, a due proportion of all the coloured rays to form a pure white, which has been commonly called, by chemiſts, a melting heat. The intenſeneſs of this light depends much upon the denſity of the heated body; for, while metals, heated to this degree, throw out a ſtrong light, the vapour at the end of the flame of a blow-pipe, properly applied to a lamp, is not viſibly luminous, though the heat be ſo great as immediately to give a white heat to glaſs. The colour of this light is affected

affected by the colour of the ignited matter. While *zinc* is calcining, the pure white *calx* throws off a light, which vies with that of the Sun in brightneſs and purity; the green *calx* of copper gives to the flame of a fire, in which it is calcining, a beautiful green; and tallow burning in a candle, being converted into empyreumatic oil, as it paſſes off from the wick, the yellow colour of this oil gives a yellowneſs to the flame, which very much alters the colours of objects ſeen by candle-light from what they appear to be in the day.

The light produced by the decompoſition of bodies in inflammation is totally independent of the heat, and its colour is blue; for ſubſtances which burn, without producing 600° of heat of FAHRENHEIT's thermometer, give light during their inflammation. Thus, *phoſphorus* of urine expoſed to the air burns and is decompoſed, producing light with very little heat; and that this is a true inflammation and decompoſition appears from this experiment. Take a receiver of white glaſs, capable of holding ſix or eight gallons; put into it a drachm of *phoſphorus* of urine, finely powdered, and half an ounce of water; cork the mouth of the receiver, and tye it over with a bladder, ſo as to exclude the external air; incline the receiver to all ſides gently, and afterwards ſet it to reſt; the powder will adhere to the ſides, and the water will drain from it. As ſoon as the water is ſufficiently drained off, the particles of the *phoſphorus* will become luminous, and emit a thick ſmoke: this will continue for ſome days; but at laſt no more light or vapour will

appear. Open the receiver, and you will find, that the air will have contracted, as it does from the inflammation of a candle in VAN HELMONT's experiment; that is, about a twentieth part. It is become unfit for inflammation; for if a lighted candle be immerfed in it, it will be extinguifhed as well as the *phofphorus*, and an animal will be fuffocated by it. The air then has fuffered the fame change, as that air which has ferved for the inflammation of other bodies; and the *phofphorus* is partly decompofed, the water in the receiver being impregnated with its acid, and the air faturated with its phlogifton. Blow frefh air into the receiver, and the light and fmoke will immediately re-appear. In like manner, it is known, that fulphur will burn and give light, without heat fufficient for ignition. Take a piece of iron heated nearly red hot, and throw a little gunpowder upon it. If the heat be of a proper degree, the fulphur will burn off with a blue flame, without heat fufficient for ignition; for, if fuch heat had been produced, the gun-powder would certainly have taken fire, which it does not. It is the inflammation and decompofition of the fulphur, and not its evaporation, which produces the light; for, if we fublime fulphur in clofe veffels, made of the moft tranfparent glafs, no light will be vifible, except at the very beginning, when a fmall portion of it burns till the air in the veffel be faturated, and rendered unfit for inflammation.

That the light, which is produced by the decompofition in inflammation, is blue, in whatever degree of heat the

the inflammation takes place, appears from obſerving the bottom and lower part of the flame of a candle, where the inflammation is; the light produced is blue. Or take a candle which has burnt for ſome time, extinguiſh it by applying tallow to the wick, and let it ſtand to cool; afterwards ſet it on fire by the flame of another candle: at firſt, no more vapour will ariſe than can be acted upon by the air at once; inflammation, therefore, will go on in the whole ſmall flame, and it will be blue. It may be neceſſary to obſerve here, that, when a candle burns, the following proceſs happens. The tallow boils in the wick, and is converted into empyreumatic oil, riſing from it in the form of vapour. As it riſes from every part of the wick, the volume is increaſed till it comes to the top, and gives to the lower part of the flame the form of a *fruſtum* of an inverted cone. The air is applied to the outer ſurface of the column of vapour, and, there decompoſing the empyreumatic oil, produces heat and blue light; the *ſtratum* of vapour, within the outer burning ſurface, is heated white-hot; the heat diminiſhes towards the center, which, if the flame be large, is ſcarcely red-hot; as the column riſes, decompoſition taking place conſtantly on its ſurface, it neceſſarily diminiſhes, and the upper part of the flame is conical. That the tallow boils in the wick can be ſeen; that it is converted into empyreumatic oil is proved by drawing the vapour, riſing in the middle of the flame, where it does not burn, into a glaſs tube; the empyreumatic oil condenſes. This alſo ſhews, that the flame does not burn in the middle. That the heat is

 produced

produced on the outer ſurface appears, if we take a ſmall rod of glaſs, and put the end of it in the blue flame on the ſurface; it will be heated white-hot and melt. Immerſe the rod into the flame, ſo that the point ſhall be in the center, it will melt and bend, where it is in the blue flame on the ſurface; whereas, if the flame be large, the point which is in the center will hardly be heated red-hot. That the empyreumatic oil is decompoſed is proved by burning a candle with a very ſmall wick in diſtilling veſſels, no condenſation of empyreumatic oil takes place. We may conclude, therefore, that light is produced by the decompoſition, as well as by the ignition, in inflammation.

I will not take up the time of this learned Society in applying theſe principles to the explanation of various appearances in burning bodies; or ground upon them any practical rules for producing ſtrong or faint, white or coloured light, theſe being ſufficiently obvious. I have choſen to illuſtrate this ſubject by experiments that may be the moſt eaſily tried; but, leſt the manner of powdering *phoſphorus* ſhould not be known, I will give the proceſs. Take *phoſphorus* of urine two drachms; put it into a four-ounce phial; pour upon it three ounces of water; heat it gently, by immerſion in warm water, till the *phoſphorus* melts; ſhut the phial with a cork; take it out of the water, and ſhake it briſkly till it be cold; the *phoſphorus* will be found in powder.

XXXI. *Ex-*

XXXI. *Experiments on ignited Bodies.* *By* John Roebuck, *M. D. F. R. S.*

Extract of a Letter from Dr. ROEBUCK *to Dr.* BROCKLESBY.

R. Feb. 15, 1776.

MR. BUFFON asserts, that he found a ball of iron, which weighed 49 lbs. 9 oz. when cold, to weigh, when heated to a white heat, 49 lbs. 11 oz. which is an augmentation of weight of $19\frac{1}{5}$ grains to the pound.

This extraordinary fact, circumstantially narrated by the very eminent and ingenious M. BUFFON, being contrary to the opinions of those philosophers who have most enlarged our natural Knowledge by their candid and cautious inquiry into the qualities of bodies, made me very solicitous to make similar experiments.

Some time ago, when I was at Birmingham, I had very luckily an opportunity, by the aid of two accurate balances of my friend Mr. BOLTON'S; one of which would, without straining the beam, weigh a pound and turn with one-tenth of a grain; and the other weigh half an ounce, and turn with the hundredth part of a grain.

 I heated

I heated a piece of iron, of nearly one pound weight, to a white heat, or what the ſmiths call a welding heat, and found, by the moſt accurate experiments which I could make, and which I again and again repeated, that the iron, when left ſeveral hours in the balance to cool, weighed nearly one grain leſs when cold than when hot; and that a piece of iron, of about five pennyweights, which was tried by the ſmaller and more accurate balance, weighed, as appeared by an index which moved oppoſite to a quadrant, ſomewhat more when cold than when hot. I tried the ſame experiment on copper; but, to my great ſurprize, I found a piece of copper, of nearly one pound weight, four grains lighter after it had been left ſome hours to cool in the balance than when it was put in. I repeated the experiment, and found the event the ſame; but ſuſpecting this might poſſibly ariſe from the copper caſting ſcales, I placed a ſheet of white paper under the balance, and collected as many ſcales as made up nearly the deficiency of the weight.

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR, London, May 9, 1776.

R. May 9, 1776. I BEG leave to inform you of the following experiments, which were made to aſcertain the variation of the weight of bodies when hot and cold. April

April 29, 1776, I heated a cylinder of wrought iron, which weighed fifty-five pounds, to a white heat, and exactly balanced the ſame, when hot, in the preſence of the hon. HENRY CAVENDISH, NATH. PICGOT, MATTHEW RAPER, ANDREW CPOSBY, EDW. DELAVAL, HAMILTON, DAVID HARTLEY, WILLIAM RUSSEL, Eſquires; Doctors HUNTER, BROCKLESBY, MORTON, WILLIAM FORDYCE, GEORGE FORDYCE, RUSSEL, WATSON junior, MUSGRAVE; Meſſ. JOHN HUNTER, BEN. WILSON, JAMES RUSSEL, RAMSDEN, WHITEHOUSE, MAGELLAN. After the cylinder had been two hours cooling in the ſcale, I weighed it again, and found that it had increaſed in weight three pennyweights and a few grains. Five hours after cooling, Mr. MAGELLAN weighed it, and found it had increaſed in weight three pennyweights ſeventeen grains. Six hours after cooling, when the cylinder was blood-warm, I weighed it again, in the preſence of Dr. HUNTER, Dr. BROCKLESBY, Mr. GRAY, and Mr. NEISBIT, and found it to have increaſed in weight four pennyweights. The day following, about twenty-two hours after cooling in the preſence of MATTHEW RAPER and ANDREW CROSBY, eſquires, I again weighed it, and found that it had increaſed in weight ſix pennyweights ſeventeen grains. Mr. ABRAM WHITEHOUSE, who was very ſolicitous to have the above experiment made accurately, procured from Mr. SAMUEL READ a very exact beam, which readily turned, to the conviction of all the above gentlemen, with leſs than a pennyweight, though loaded with the above iron cylinder; but MATTHEW RAPER, ANDREW CROSBY, Eſquires, and myſelf

ſelf examined the beam leiſurely and accurately, and found it turned very diſtinctly with four grains, though loaded as above. In order to diſcover the cauſe of this increaſe of weight of the cylinder when cold, I heated two ounces eight pennyweights of the ſcales or *calx* of wrought iron, and found the ſame to increaſe in weight five grains when cold.

I heated two pieces of pure ſilver which weighed two pounds, ten ounces, five pennyweights; and when the ſilver was cold it increaſed in weight five grains, though it produces no *calx* from being heated red-hot.

The above experiments are conformable to thoſe which I formerly made with ſome very exact ſcales at Birmingham; an account of which I tranſmitted to Dr. BROCKLESBY.

I have the honour to be, &c.

XXXII. *Experiments and Obſervations on a new Apparatus, called, A Machine for exhibiting perpetual Electricity. In a Letter to the Rev. Dr.* Horſley, *Sec. R. S. from Mr.* William Henly, *F. R. S.*

REVEREND SIR, March 4, 1776.

R. May 16, 1776. MY ingenious friend Mr. GEORGE ADAMS, philoſophical inſtrument-maker to his majeſty, lately put into my hands a little apparatus, which he called a machine for exhibiting perpetual electricity, and informed me, that it was the invention of ſome foreign gentleman (a). This machine conſiſted of a circular plate of glaſs, about eight inches in diameter, covered on one ſide with a coating of bees-wax and roſin, about the ſixteenth part of an inch thick. This coat of wax, &c. being ſtrongly excited with a dry warm flannel, he placed upon it a circular board, of the ſame dimenſions, coated with tin-foil, and furniſhed with a glaſs handle ſcrewed to, and ſtanding upright upon it. Theſe bodies having remained in contact ſome ſeconds, the board was raiſed up by the glaſs handle; when, applying the knuckle to the tin-foil coating, a ſnap was heard, a ſpark ſeen, and a ſmall ſenſation felt. On replacing the board, and permitting it to remain ſome ſeconds, as before, having touched the tin-foil with a finger, on removing it again, and applying the knuckle, as at firſt, the ſame

(a) I have ſince learned from Mr. NAIRNE, that M. VOLTA, of Coma, near Milan, was the inventor of it.

phenomena

phenomena were produced; and might, Mr. ADAMS obſerved, be repeated for a long time, without any renewal of the excitation of the wax, any farther than the replacing the board might be ſaid to excite it. It immediately occurred to me, that, as this plate of wax, &c. was made by excitation, a ſtrong negative electric, the phenomena produced by it could only be the reverſe of thoſe I had formerly made with an excited plate of glaſs, and publiſhed in the Phil. Tranſ. vol. LXIV. part II. p. 407.; *viz.* where mine were poſitive, theſe were negative; and where mine were negative, theſe were poſitive. But, to determine this matter, I made the following experiments. Firſt, I inſulated Mr. CANTON's electrometer, and having electrified the balls poſitively, I preſented toward them the excited wax, as ſoon as it had been ſeparated from the coated board; and perceived, as I expected, that the balls were attracted by the wax; but, if the balls were electrified negatively, they were as plainly repelled by it. The board produced juſt the contrary effect. Secondly, I held my Leyden *vacuum*, or analyſis of the Leyden bottle, deſcribed Phil. Tranſ. vol. LXIV. part II. p. 400. by the coated bulb, and touched the braſs ball on the neck of it with the coated board, the moment it had been ſeparated from the excited wax, &c. and inſtantly perceived a variety of beautiful ſtreams dart from the point of the wire in the bottle, and ſpread themſelves in different directions through the bulb. On repeating the experiment, and preſenting the coated part of the bottle toward the board, a ſmall ſpark of light appeared upon the point of the incloſed wire; a plain indication

indication that the point had received electricity, and, as before obſerved, that the coated board, being ſeparated from the wax, &c. was ſtrongly electrified *plus*; and conſequently, the coating of wax, &c. on the plate of glaſs *minus*. Theſe phenomena, being ſo often produced, without a freſh excitation of the wax, though they are aſtoniſhing to ſtrangers, will not be ſo ſurprizing to electricians, who have conſidered Mr. GREY's experiment with a cone of ſulphur, contained in a glaſs veſſel, which, as often as they were ſeparated, ſhewed ſigns of electricity in all ſtates of the weather. See Dr. PRIESTLEY's Hiſtory of Electricity, 2d edit. p. 39. I have ſhewn at large, in a former paper, that merely heating either glaſs or amber will not make them electrical; but the friction of glaſs againſt glaſs, or ſealing-wax againſt ſealing-wax, previouſly warmed, I find, will excite either of theſe ſubſtances; and my ingenious and learned friend THOMAS RONAYNE, Eſq. informs me, that he had long ſince made the ſame remark on ſealing-wax. But, preſſing a finger in the gentleſt manner on the amber, after heating, will excite it. Indeed, a fine piece, which I frequently carry in my pocket, I always perceive to be electrical, without any other friction than what it receives from the pocket. Sealing-wax, Mr. RONAYNE tells me, he always found to be affected in the ſame manner; and negative electrics, *per ſe*, being once thoroughly excited, are obſerved to retain their electrical quality very long, as they do not ſo ſoon attract the moiſture in the atmoſphere as glaſs. Glaſs, however, will retain its electricity many hours, as I have had frequent occaſion to remark. My late friend

Mr. CANTON informed me, that, having excited a rod of glaſs very ſtrongly, he ſet it at ſome diſtance from the fire in his parlour, and found that it was electrical, after ſtanding in that ſituation, in dry air, twenty-four hours. How much longer it would have retained its electricity, had he let it remain there, he knew not. How long a large and neatly-prepared Leyden bottle will retain its charge, ſo as to be ſenſibly electrical, I have never experienced; but Dr. PRIESTLEY obſerves, Hiſtory of Electricity, p. 516, that he has more than once received ſuch ſhocks as he ſhould not like to receive again from the *reſiduum* of his battery, even two days after the diſcharge, and when papers, books, his hat, and many other things, had lain upon the wires the greateſt part of the time. Even the *reſiduum* of a *reſiduum*, he ſays, he has known to remain in his battery many days (a). One thing, however, is very remarkable in Mr. ADAMS's apparatus, *viz.* ſuppoſing the negative electric to have parted with its electricity to the rubber; why, when the coated board or plate of metal is ſet upon it, and that plate is touched by a finger, the equilibrium is not thus preſently reſtored? But, perhaps, when the electric matter, naturally inherent in bodies, is once thoroughly excited and put in action,

(a) My friend the reverend Mr. HEMMING, hath been ſo obliging as, at my requeſt, to make a variety of experiments, with a view to determine this matter, and ſhewed me a ſmall bottle, which attracted a thread of trial at one-ſixteenth of an inch diſtance, May 23, though the bottle had been charged and ſtood in a cupboard in his ſtudy from March 14, *viz.* 70 days. The cylinder to his electrical machine will alſo ſeparate the balls of Mr. CANTON's electrometer a fortnight after uſing, though a variety of methods have been repeatedly uſed to deſtroy that power in the interval.

action, it is not so soon as might be suspected reduced again to a quiescent state, especially in bodies so peculiarly adapted to affect each other as these appear to be. Mr. LANE has favoured me with a very curious experiment, which he made as long since as the month of June 1764, and then shewed to many of his friends, which seems fully to confirm this opinion. I have, therefore, requested his leave to insert it, as follows. Having procured two large pieces of thick plate glass A and B, with plain surfaces, and fitted them so as to coincide with each other, he coated a part (about eighteen square inches) of A, on one side, with tin-foil, and an equal part of B he coated in the same manner, so as to answer exactly to A, leaving a margin of glass, an inch and a half broad, in the narrowest part; but, at one of the ends of each plate (which end was reduced in breadth), not less than five inches of the glass were reserved uncoated, for the purpose of handling them. The uncoated sides of these glasses being laid together, they were charged by the machine as one plate; when the plate A, which touched the prime conductor, was found, on separating them, to be positive on both sides; and B, which was touched by a finger during the operation, was negative on both sides. Then, laying them in contact, as at first, and making the discharge as with the Leyden bottle, the plates were still found to cohere, and after separation were observed to remain strongly electrical; but with an electricity directly contrary to that they shewed before the discharge, A being now negative, and B positive on both sides. But, what is particularly to my purpose, if the

coating on A and B (after laying them again together as at first) were touched, at the fame time, by a thumb and finger of the hand, or any conductor communicating with the earth, the plates would then, on being feparated (the experiment being made in a dark room) emit a ftrong flafh of light; and this phenomenon Mr. LANE has frequently produced twelve or fourteen times fucceffively, touching the coating of the plates each time before the feparation, without renewing the charge in the glafs by the machine; but if he omitted to touch the coatings as above mentioned, no light was vifible on the feparation of the plates (b). Should thofe gentlemen, if any fuch remain, who are of opinion, that in electrical experiments two fluids, the vitreous and the refinous, are concerned, proceed to make experiments of this kind, they may, perhaps, from fome phenomena, be induced to draw conclufions which they may think not unfavourable to their own hypothefis.

My experiments with the excited plate of glafs, publifhed in the Philofophical Tranfactions, as before mentioned, may ferve, however, as a key to explain both Mr.

(b) Crown-glafs, that is, the glafs commonly ufed for fafh-windows, though fo much thinner, fucceeds in this experiment as well as the plate-glafs; but what is very remarkable, the Dutch plates, when treated in the fame manner, have each a pofitive and a negative furface, and the electricity of both furfaces, of both plates, is exchanged for the contrary electricity in the difcharge. If a clean, dry, uncoated plate of looking-glafs be placed between the coated looking-glafs plates, or between the plates of crown-glafs, it appears, after charging, to be negatively electrified on both fides; but if it be placed between the Dutch plates, it acquires, like them, a pofitive electricity on one furface, and a negative electricity on the other. Further particulars, with a defcription of fome new electrical apparatus, conftructed on account of thefe phenomena, will be given at another opportunity.

GREY's experiments, and thofe made with Mr. ADAMS's little apparatus now under confideration. Having procured a plate of glafs, ten inches long and eight inches broad, coated in the manner of Mr. ADAMS's, I was inclined to purfue thefe inquiries fomewhat farther. Accordingly, I placed, upon a ftrong fupporter of glafs, a circular board, with a fmooth and flat furface; and upon this board I laid a circular brafs plate, of nearly the fame fize; and laftly, I placed upon the brafs plate Mr. CANTON's electrometer. Then, having excited the plate of wax with dry, warm flannel, experiment 3, I fet it upon the infulated apparatus. The balls prefently opened, and, on examination, appeared to be electrified negatively; but, on removing the wax, they clofed, and opened again much wider, and were then found to be electrified pofitively. In this experiment, the quantity of electricity, naturally inherent in the balls, ftrings, &c. had been drawn up into the apparatus by the attractive power of the excited plate of wax, and they were thus left in a negative ftate; but, on removing the plate of wax, the balls clofed again, in confequence of the return of the electricity, which would be increafed if the plate of brafs had been touched by a finger, &c. and the balls then became very powerfully electrified *plus*. By applying, in the fame manner, the excited uncoated plate of glafs, or the excited uncoated fide of the fame plate, the reverfe of thefe phenomena took place, as I have before defcribed them and referred to in the beginning of this paper.

Experiment 4. I infulated two of Mr. CANTON's electrometers, A and B, and having raifed them in fuch a manner.

manner as to let the balls hang about the eighth part of an inch higher than the plate of braſs on which the excited plate of wax was laid, I electrified the balls of A poſitively, and the balls of B negatively, ſo as to diverge about an inch; I then brought the inſulated apparatus as near as I could to the balls, without affecting either, (the braſs plate might then be at nearly an inch and an half diſtance from the neareſt ball, both of A and B); then, ſuddenly removing the excited wax, the balls of B inſtantly flew to the braſs plate, and thoſe of A were, at the ſame inſtant, repelled to as great a diſtance from it. The apparatus having remained in this ſituation ſome ſeconds, on withdrawing the ſtand with the braſs plate, &c. the balls of B cloſed, having received by this proceſs the quantity of electricity they had before been deprived of; but the balls of A ſtill remained ſeparate, as wide as ever.

Experiment 5. Having replaced the excited wax, &c. upon the braſs plate, I again electrified the balls of A and B, as in the former experiment, *viz.* thoſe of A poſitively, and thoſe of B negatively. I then took a ſmall phial, properly prepared for the Leyden experiment, containing only about three ſquare inches of coated ſurface; then, preſenting the knob, on the wire of the phial, to the plate of braſs, I removed the wax, &c. and inſtantly ſaw a ſtrong ſpark between the braſs plate and the knob of the phial: when, preſenting that knob towards the balls of A, they were conſiderably repelled; but on preſenting it toward thoſe of B, they were as much attracted. I have made ſeveral other experiments with this apparatus; but, as they all agree with thoſe

above

above mentioned, I think it unneceffary to recite them. I have likewife omitted to give a drawing; as to electricians, I apprehend, this paper will be intelligible without one; and to thofe who have not confidered the fubject, I imagine, it would be of very little, if indeed of any ufe whatever. The fame difficulty which occurred to Dr. FRANKLIN, in his analyfis of the Leyden bottle, may be faid to occur alfo in this apparatus, *viz.* it is hard to fay how, or where this electricity is depofited, there is fo much of it; and it is fo eafily put in action, that I am ftill further confirmed in an opinion that I have long entertained, *viz.* that the flighteft friction between bodies of every kind, in every fituation, may difturb the electric matter contained in them, though this effect be imperceptible to us, having no electrometer nice enough to difcover it. I am, &c.

In the month of March laft, I repeated Mr. GREY'S experiment with the cone of fulphur and the glafs; and find that, on feparating thefe bodies, the fulphur hath hitherto (c) always acted as a ftrong negative electric. Mr. WILCKE, in repeating this experiment, obferved, that if the glafs veffel, into which the fulphur was poured, was covered with a coating of metal, the electrical property of the two bodies would be increafed, the fulphur having acquired a ftronger negative, and the glafs a ftrong pofitive electricity (d).

(c) Sept. 23, 1776.

(d) The ftem of the glafs fhould be varnifhed, or covered with cement, and the cone of fulphur (as M. EPINAS hath directed) be provided with a glafs handle, that the refpective bodies may be feparated at pleafure, without touching them.

I have

I have lately ſeen a very neat apparatus, much ſmaller than that I have mentioned in this paper, made by Mr. NAIRNE, the coated plate of glaſs meaſuring only three inches in diameter. With this apparatus I made the following experiments. I inſulated two of Mr. CANTON's electrometers, A and B, and having excited the coating on the glaſs plate, I ſet upon it the plate of metal, and having permitted it to remain in that ſituation about half a minute, I raiſed it up by the glaſs handle, having firſt preſſed it cloſely into contact, and placed it upon the electrometer A. The excited electric I placed in like manner upon B. The balls of both the electrometers diverged conſiderably; thoſe of A poſitively, and thoſe of B negatively. Then, removing the excited plate of wax, &c. from B, the balls cloſed, and opened again poſitively, upon the principle already explained in the preceding paper. If, inſtead of the braſs plate, the plate of glaſs was excited (that is, the uncoated ſide of it), and placed upon the electrometer A, the balls were affected in the very ſame manner (differing only in the degree of power) as thoſe I have before mentioned in my experiments with the excited uncoated plate of glaſs; Phil. Tranſ. vol. LXIV. part II. p. 407.

A variety of new experiments and obſervations, relative to ſeveral articles mentioned in this paper, and other new facts in electricity, particularly the electricity of chocolate, and the reſtoration of that property of it, when loſt, by melting it, with the addition of a ſmall quantity of olive oil, will be preſented to the Royal Society, as ſoon as the materials are properly digeſted and tranſcribed.

XXXIII. *Account*

XXXIII. *Account of the Iron Ore lately found in* Siberia. *In a Letter to Dr.* Maty, *Sec. R. S. by* Petr. Simon Pallas, *M. D. F. R. S.*

S I R,

Peterſburg. Nov. 6, 1775.

R. May 16, 1776.

THE travels in which I have been employed, by order of our empreſs, ſince the year 1768, have interrupted the correſpondence I had the pleaſure to entertain with ſome of the Fellows of the Royal Society of London, particularly the worthy Mr. COLLINSON; and as this ingenious man, in the mean time, has left this world, I make ſo free as to addreſs myſelf to you directly, for the leave of communicating from time to time, to the Royal Society, ſuch obſervations or papers, which I am not bound to deliver to the Academy here. I would have before this obſerved that duty, to which the honour of being a foreign member of the Royal Society obliges me, had not the diſtance in which I have lived theſe ſeven years, moſtly out of Europe, and the troubleſome manner of travelling in theſe countries, together with the diſtractions and duties of my employment, rendered it impoſſible. Being now returned to a more quiet manner of living, I ſhall never neglect an opportunity of ſhewing my attachment for the

the Royal Society, as well as the highest respect to that learned Body.

I have embraced the opportunity of a parcel I sent to Mr. DRURY, to offer the Society a specimen of the native iron, of which I found such a large mass in the Siberian mountains, which actually is transporting to Petersburg. I read in some foreign journals, that a short account of this mass has been published in the last volume of Phi-

MISCELLANEA of our Academy; but as the contents of it, drawn from the informations I gave to our Academy in my itinerary relations, seem not to have been exact, I beg leave to give you here a faithful and fuller account of the place and circumstances, in which that memorable mass was found.

It is to be observed, that in the neighbourhood of the river Jenisei, one of the largest, that runs from the South through Siberia and to the Northern Ocean, and near which the mass of native iron has been detected, there is great plenty of iron ores, as well in the flat layers towards the Northern level of the country, where, amongst others, whole banks of *ochraceous* minerals, with scattered trees and pieces of wood turned to rich iron ore; and near the town of Jeniseisk, a rich iron ore, in the form of white clay and white sparry stones, is to be found, as also in the steep mountains, where the *strata* dip very considerably, and ores of iron, copper, and even impregnated with gold, are found in veins and nests. On the mountains, that lie along the Eastern

side

side of the abovementioned rivers, from 56° to 52° of latitude, where the highest ridge of mountains begins, iron ores are most common, and the mountains generally consist of grey or black slates and shivers, which rise steeper, or in a greater angle to the horizon, as they come nearer to the high ridge of mountains, and approach more to a level position, as they extend to the North. Some of these secondary mountains are very high, rising very often to some thousand feet above the sea surface, and most of them are covered with forests. A very rich iron ore in veins was here discovered in the year 1749, on a steep, woody mountain, about ten English miles from the river Jenisei, and 180 miles from the town of Krasnojarsk, situated on that river to the Southward, about 54° of latitude, between two rivulets, known by the names of Ubei and Sifim, and running into the river on the Eastern side. This place was then visited by the Russian miners; but as there is plenty of iron ores situated much nearer to the Fabricks, this mine never was worked, though the ore contains above seventy pounds of iron in the hundred weight, being of a dark steel colour, turning red when rubbed, and in some parts endowed with a magnetic virtue. Upon the same mountain, where this mine is situated, on the North side, not much below the top of the mountain, the mass of native iron lay on the very ridge, without being fixed to the rock, which is a grey stratified *saxum*. There was on that and the neighbouring mountains no trace of ancient miners and their kilns, which are found in many other

parts of Siberia, and in which thofe miners, of fome former and hitherto unknown nation inhabiting thefe parts, moftly worked upon copper ores. Nor could fo large a mafs ever have been formed in the fmall kilns of thefe people, which never could yield more than 50 or 60 pounds of metal at a time; whereas this mafs, in its firft condition, weighed above 1680 Ruffian pounds. It is throughout of the nature you may fee in the fpecimen which M. DRURY will deliver to you. The iron is formed in a coarfe, fpungy texture, moftly pure, perfectly flexible, and fit to be worked to fmall tools by a moderate fire; but in a more violent one, and chiefly being melted down, it becomes dry and brittle, refolves in grains, and will no more ftick together, nor extend under the hammer. In its natural ftate, the iron itfelf is incrufted with a kind of varnifh, which has preferved it from ruft; but, wherever this is loft, or the iron bars broken, ruft comes on very readily. The cavities formed by the iron are equally filled up with a kind of *fluor*, which for the moft part is of a clean, tranfparent, amber colour, cuts glafs, has none of the properties of *fcoria*, and forms, according to the hollows it fills, various roundifh grains or drops, very gloffy and clean, on their furface, having one or more flat furfaces. This *fluor* is extremely brittle, and thus, by cutting off any part of the mafs, this fubftance is loft, and comes off partly in grains, and partly in form of a coarfe powder of vitrefcent matter. The whole mafs has no regularity of form, but refembles a large, oblong, fomewhat flattifh pebble, and is coated on the outfide

 with

with a matter reſembling ſome blackiſh, brown iron ores. This coat, however, covers not the whole maſs; it is alſo very rich of iron, and even the tranſparent *fluor* yields ſome pounds of iron in the hundred. Whoever will conſider the maſs itſelf, or large ſpecimens of it, will not have the leaſt doubt of its being worked by nature, ſince it has no one character of *ſcoriaceous* matters melted by artificial fire, or commonly found among volcanos.

With regard to theſe, as ſeeming a probable place where this maſs could have been formed, it may not be amiſs to add the following obſervations. The mountains, where it was found, are part of the Northern extenſions of that mighty chain of mountains which runs from Weſt to Eaſt through Aſia, and forms the natural limits of Siberia, with the Deſarts of Tartary, the Mongols, and the Chineſe Empire. From the river Irtiſh, where the forehills and lower parts of theſe mountains yield, in a great many places, the richeſt ſilver ores, the chain runs generally ſomewhat to the North-eaſt, and therefore extends to the Eaſt of the river Jeniſei, over a much greater part of Siberia than what it did before. Its forehills are almoſt every where compoſed of rocks and *ſtrata*, riſing very ſteep to the horizon, and the horizontal layers are only found in the level country, in which alſo all kinds of foſſil and petrified ſea productions are very ſcarce, and only found in the very Northern parts of Siberia. Common flint is as ſcarce in Siberia as petrifactions, and nothing like productions of volcanoes any where to be found. Even in ſome places, where hot ſprings are

found,

found, these seem only due to collections of *pyritae* of no great extent, and the slight earthquakes which are sometimes observed about the river Irtish, and more frequently about the lake Baikal, certainly rise in the very neighbourhood of this lake and of the Noor Saissan, which gives rise to the river Irtish; and about these lakes never any thing like a volcano has been heard of, nor is there one known in the Northern part of Asia, except those in Kamtschatka and the islands newly-discovered between that peninsula and the continent of North America. The same may be assured of the Urallian mountains, a ridge that runs from South to East, and continues to the very Northern Ocean and Nova Semlja, being only interrupted by the Streight of Waygat. It is this ridge of mountains that makes the natural limit between Europe and Asia, and to the East of which the largest share of true remains of elephants, rhinoceroses, and large buffaloes, is found in the banks of all the larger rivers, that run from the above-mentioned chain of mountains to the Northern Ocean, and yield such remains from the places where they reach the plains of Siberia (no such bones being ever found in the higher mountains) to the very Ocean; where the frozen earth of the Northern plains preserves these remains of Southern animals in such perfection, that when I was at Irkuzk, the head and two legs of a true rhinoceros were sent from the river Wilui, with its skin and part of the tendons preserved on them, which are now in the Museum of our Academy, and fully described

described and figured in the XVIIth volume of *Novæ Commentaria Petropolitana*.

By the very first ships that will sail from this port in spring, I shall take the liberty to send you, for the Royal Society, whatever I have published since 1767, and some other curiosities for the Museum. If any particular production, or account of natural productions from these parts, should be wanted to the Society, I shall be ready to serve with whatever I am able to supply.

XXXIV. On

XXXIV. *On the Cryſtallizations obſerved on Glaſs.* *By* James Keir, *Eſquire, of* Stourbridge. *Communicated by* G. Fordyce, *M. D. F. R. S.*

R. May 23, 1776.

THE peculiar figure of rock-cryſtal has been long obſerved. Many other ſubſtances, as ſpars, precious ſtones, pyrites, ores, metals (a), ſalts, water (b), and oil (c), are alſo known to affect an uniformity of ſhape, when they are expoſed to certain degrees of heat, cold, fluidity, and other neceſſary circumſtances. From their reſemblance in this reſpect to rock-cryſtal, they are ſaid, when they aſſume their peculiar forms, to cryſtallize; and the regularly-ſhaped bodies, into which theſe ſubſtances concrete, are alſo called cryſtals.

(a) Native gold has been found in a cryſtallized form. M. ROME DE L'ISLE, in his *Eſſai de Cryſtallographie*, p. 390, ſays, that he has ſeen pieces of native gold which were eight-ſided ſolids, like cryſtals of allum, and one piece which was an hexagonal plate. In Dr. HUNTER's muſeum, ſome fine ſpecimens of cryſtallized native gold are to be ſeen. Gold may be cryſtallized by art alſo. Some æther having been poured into a ſolution of this metal in *aqua regia*, I obſerved, a few months afterwards, the gold ſeparated from the *menſtruum*, in the form of diſtinct polygonous priſms.

(b) The various and regular forms of the particles of ſnow, which is nothing elſe than water cryſtallized, are well known.

(c) The cryſtals, formed by cold, in the oil of ſaſſafras, have been obſerved to be very beautiful, regular, hexagonal priſms.

In

In many ſubſtances, when broken, the parts appear to have ſome determinate figure. This determination of figure, or grain, as it is called, is obvious in biſmuth, regulus of antimony, zinc, and all other metallic bodies, which may be broken without extenſion of parts; and although the ductility of gold, ſilver, lead, and tin, prevents the appearance of the peculiar grains, when pieces of theſe metals are broken, yet we have reaſon to believe, that, by expoſing them to proper circumſtances, they alſo would ſhew a diſpoſition to this ſpecies of cryſtallization, as it may be called, by a further extenſion of that term; for Mr. HOMBERG has obſerved, that when lead is broken while hot, in which ſtate it is not ductile, a granulated texture appears. Perhaps all homogeneous bodies, in their tranſition from a fluid to a ſolid ſtate, would, if this tranſition were not effected too haſtily, concrete into cryſtals, or bodies ſimularly figured. Inſtances of ſuch cryſtallization have occurred to me in glaſs, which had paſſed very ſlowly from a fluid to a ſolid ſtate; and the form, regularity, and ſize of theſe vitreous cryſtals have varied according to the circumſtances with which their concretion had been accompanied. I ſend along with this paper a few ſpecimens of this cryſtallized glaſs, together with a drawing of ſome of the moſt remarkable cryſtals.

The pieces of glaſs, marked N° 1. were taken from the bottom of a large pot, which had ſtood in a glaſs-houſe furnace at the time the fire was allowed gradually to extinguiſh. In this caſe, the maſs of heated matter was ſo great that, without the addition of fuel, the heat con-

tinued long, and the tranfition of the glafs from a fluid to a folid ftate was very flowly accomplifhed. The upper part of this glafs was found to be changed into a white, opaque, or rather femi-opaque fubftance, refembling in colour and texture fome of the white fpars. Under this cruft, which, in fome places, was a quarter of an inch thick, and in others more, the glafs was tranfparent, but confiderably obfcured, and its colour was changed from a dark green to a dull blue. In this femi-pellucid glafs were difperfed many white, opaque, regular cryftals, the form of which was generally that of a folid, whofe fide-view is reprefented by fig. 1. and whofe bafis by fig. 2. The furface of thefe cryftals feems to be bounded by lines rather elliptical than circular, which are fo difpofed, that a tranfverfe fection of a cryftal, that is, a fection perpendicular to its axis, is an hexagon, as is fhewn in fig. 3. and 4. the former of which reprefents a view, and the latter a plan, of that fection. In the middle of each bafis of the cryftal, a conical cavity appears, as is fhewn in fig. 1. and 2. The elliptical lines which bound the furface of the cryftals feem to be occafioned by the edges of many thin plates, fo arranged round the axis of each cryftal, that their longitudinal diameters are parallel to that axis. Of thefe plates, twelve are larger, more confpicuous, and better defined, than the reft. They are placed in pairs, at an equal diftance from each other, forming the fix angles of the hexagonal fection and bafis, as appears in fig. 1. 2. 3. and 4. The intervals between the pairs of plates, that is, the areas of the triangles into which the hexagonal

hexagonal ſection is divided by theſe pairs, are filled up partly by ſmaller plates affixed to the ſides of the principal plates, and of each other, at an angle of 60°, and partly by a ſubſtance ſomewhat leſs opaque and darker-coloured than that of the plates. The ſize of the contiguous and of the neighbouring cryſtals does not vary much, although that of cryſtals, found at different depths of the ſame pot, were obſerved to differ conſiderably. The greateſt diameter of the cryſtals, from which the figures 1. 2. 3. and 4. were copied, was about $\frac{1}{20}$th part of an inch, ſo that theſe figures repreſent the cryſtals conſiderably magnified. All the cryſtals are not by any means formed with the ſame exactneſs as thoſe deſcribed; ſome having the hexagonal form leſs diſtinctly marked; but the regularity of moſt of them is ſo obvious, that no doubt can remain of the perfection of the cryſtallization.

Another kind of vitreous cryſtallization appears on the piece of glaſs marked N° 2. which was taken from the bottom of a pot, that had been pulled out of the furnace, while the glaſs was red-hot. The cryſtals are of two kinds; thoſe repreſented by fig. 5. are of the columnar form; their altitude is about one-eighth of an inch, and the diameter of their baſes about one-fifth part of their altitude; their ſides ſeem to be irregularly fluted, or cut in grooves. The other kinds of cryſtals, which are repreſented by fig. 6. 7. and 8. have baſes of nearly the ſame diameter as the columnar ones; but their altitude is much leſs, being only about one-ſixth part of their diameter. Their baſes are bounded by lines, ſeemingly

ragged and irregular; but ſeveral of them ſhew a tendency to an hexagonal form, the regularity of which may have been diſturbed by the motion of the melted glaſs acting upon and bending theſe very thin cryſtals, while they were hot and flexible, at the time when the pot was pulled out of the furnace.

The ſpecimens marked N° 3. are pieces of a glaſs-houſe pot, down the outer ſides of which ſome melted glaſs had run, and adhered long enough for the formation of various kinds of cryſtals. The inner ſides alſo of theſe pieces are covered with glaſs variouſly cryſtallized. Some of theſe cryſtals ſeem to be ſemi-columns, of which the flat ſides, or interior ſurfaces, are expoſed to view, and are repreſented by fig. 9. Other cryſtals, repreſented by fig. 10. ſeem to conſiſt of ſeveral ſemi-columnar ones, uniting together in the ſame plane round a common center, like broad, flat ſpokes of a wheel. Many of theſe ſpokes ſeem to become narrower as they approach the center of the wheel, and, therefore, reſemble more the ſegments of *fruſta* of cones cut along their axis, than of cylinders. But, perhaps, this appearance proceeds only from the ſemi-columns being ſo diſpoſed near the center of the wheel, that the edge of one is laid over the edge of the contiguous ſemi-column, like the ſpokes of a fan.

In the ſpecimen of glaſs, marked N° 4. which had run through a crack in a pot, and had remained adhering to the bars of the grate of a furnace, ſufficiently long for a cryſtallization to take place; ſome of the cryſtals appear oblong

oblong and needle-like, and others globular, or nearly of the globular form. In this piece of glaſs, many of the needle-like cryſtals are ſeen to unite round a common center; and although they have probably been prevented, by the too ſudden cooling of the glaſs, from concreting in a ſufficient number to make complete globular cryſtals, yet they ſufficiently ſhew the manner in which thoſe, which are complete, have been formed. All the cryſtallizations, hitherto deſcribed, were obſerved in a dark, green window-glaſs, made at Stourbridge, and called Broad-glaſs. This glaſs is compoſed of ſand, kelp, calcareous earth, and lixiviated vegetable aſhes.

Cryſtallizations frequently occur alſo in the glaſs of which common bottles are made, the materials uſed in the compoſition of which are nearly the ſame as thoſe above mentioned for broad-glaſs, with the addition ſometimes of the *ſcoria* of iron furnaces. Of this kind is the ſpecimen marked N° 5. in which the cryſtals are not inveloped in a medium of tranſparent uncryſtallized glaſs, for the whole piece is an opaque, cryſtallized ſubſtance; but they are prominent from the ſurface of the maſs. The form of the cryſtals is that of the blade of a two-edged ſword, whoſe point is truncated. In no other glaſs have I ſeen ſuch perfect cryſtals as in thoſe two kinds above mentioned, broad and bottle-glaſs, which being more fluid and leſs tenacious, when melted, than any other, the minute particles of which cryſtals conſiſt, more eaſily concrete, and apply themſelves to each other with leſs reſiſtance from the medium. Perhaps alſo the

greater

greater proportion of calcareous and other earthy particles may diſpoſe theſe glaſſes to cryſtallize more than others, which contain a larger quantity of ſaline and metallic fluxes.

Flint-glaſs, when long expoſed to a dull red heat, acquires a cloudineſs, which, probably, proceeds from a number of ſmall white particles, concreted by means of cryſtallization; but theſe cryſtals are too minute for obſervation. I ſuſpect alſo, that the opaque whiteneſs, given to glaſs by arſenic, is the effect of a cryſtallization, to which this ſubſtance diſpoſes certain kinds of glaſs; for the opacity given to ſuch glaſs by arſenic, being greater than the opacity of the arſenic itſelf, cannot be communicated to a large proportion of tranſparent glaſs, merely by the mechanical interpoſition of this opaque, and ſometimes only ſemi-opaque ſubſtance.

Mr. REAUMUR has obſerved, that ſome kinds of glaſs, by long expoſure to certain degrees of heat, acquire a white opaque cruſt on their ſurface; and that this change of colour and texture, by a longer continuance of the heat, penetrates farther, till at length the whole ſubſtance of the glaſs is converted into a white, opaque body, which, from ſome ſuppoſed reſemblance to porcelain, has been diſtinguiſhed by the name of REAUMUR's porcelain, but is really nothing elſe than glaſs indiſtinctly cryſtallized.

Some of the properties of glaſs are conſiderably changed by cryſtallization; its tranſparency is deſtroyed, and it acquires an opaque or ſemi-opaque whiteneſs; its denſity is

 increaſed,

increaſed, for the denſity of a piece of cryſtallized glaſs was found, by experiment, to be to that of water as 2676 to 1000; whereas the denſity of a piece of uncryſtallized glaſs, which had been contiguous to the former, and conſequently had been compoſed of the ſame materials, and expoſed to the ſame heat and other circumſtances, was to the denſity of water as 2662 to 1000. The brittleneſs of glaſs is diminiſhed by cryſtallization; for cryſtallized glaſs is leſs apt to crack by change of heat and cold.

Cryſtallization is always accompanied or preceded by an evaporation of the lighter and more fluid parts of the glaſs; for I found, that, by expoſing a piece of tranſparent glaſs till it was entirely cryſtallized, one fifty-eighth part of its weight was loſt by evaporation: and I am induced to believe, from other trials, that glaſs, which contains too large a proportion of ſaline fluxes, is leſs capable of cryſtallizing than other harder glaſſes, till it has loſt its ſuperfluous quantity of ſuch fluxes by evaporation. A doubt may therefore ariſe, whether the change of properties, induced by cryſtallization, be merely the effects of altering the texture, that is, the arrangement of the minute integrant parts of glaſs; ſince this change is always accompanied with a loſs of the lighter parts of the cryſtallizing ſubſtance. But, although a ſuperfluous quantity of ſaline or other fluxes may impede the cryſtallization, yet, that the change of properties, induced by cryſtallization, is principally or ſolely the effect of an alteration of texture, is evident from this obſervation; that a piece of cryſtallized glaſs, when expoſed to a heat

conſiderably

confiderably more intenfe than is fufficient merely for its fufion, and afterwards haftily cooled, lofes all its acquired properties, and is again reduced to the ftate of tranfparent brittle glafs, which, however, by means of the evaporation it has fuftained of its lighter and more volatile parts, is rendered confiderably harder, denfer, and lefs fufible, than it was before the cryftallization.

Many analogous inftances might be adduced to fhew how much the properties of bodies depend merely on the different arrangements of their integrant parts, or on their modes of cryftallization. Thus, for inftance, caft-iron and fteel, when cooled fuddenly, acquire a much finer grain or texture than when annealed, or flowly cooled, and are alfo more hard, elaftic, brittle, and fonorous. From the above defcription of vitreous cryftals we learn, that very different cryftallizations occur in the fame kind of fubftance expofed to different circumftances; and even that fometimes differently-fhaped cryftals are found in the fame piece of glafs; in which cafe, the circumftances muft have been the fame. Perhaps, indeed, the difference, obfervable in the fhape of the cryftals in the fame piece of glafs, may only mark the different periods in the progrefs of cryftallization; for the cryftals reprefented by fig. 6. 7. and 8. which are found in the fame piece of glafs as thofe reprefented by fig. 5. do chiefly differ from thefe in their altitude; and perhaps the latter kind may have been compofed of a number of the former uniting by their bafes. The wheel-like cryftals alfo, fig. 10. feem to confift of feveral

of

of the ſemi-columnar one, arranging themſelves round a common center like the ſpokes of a wheel. The globular cryſtals in the ſpecimen N° 4. have been already obſerved to conſiſt of many needle-like cryſtals, converging to one central point.

Does not this diſcovery, of a property in glaſs to cryſtallize, reflect a high degree of probability on the opinion, that the great native cryſtals of *baſaltes*, ſuch as thoſe which form the Giant's Cauſeway, or the pillars of Staffa, have been produced by the cryſtallization of a vitreous *lava*, rendered fluid by the fire of volcanos?

This opinion is further confirmed by the following conſiderations. The priſmatic and other regularly-ſhaped *baſaltes* have been almoſt always found to be accompanied with *lava*, pummice-ſtones, and other veſtiges of the fire of the volcanoes, whenever they have been carefully examined by intelligent naturaliſts, as has been ſhewn by M. DESMARETS, in his Memoir on the *Baſaltes* of the province of Auvergne, in France; *Mem. de l'Acad. des Sciences*, 1771. Baſaltic columns have even been diſcovered, according to the ſame author, among the productions of volcanoes now exiſting, as of thoſe of Mount Etna and of the Iſle of Bourbon.

2. The ſubſtance of which theſe baſaltic maſſes conſiſt, is generally of the ſame nature and appearance as the neighbouring and adjoining *lava*. It is generally compact, fuſible, and of various degrees of hardneſs, probably according to the matters of which the vitreous maſs was compounded. M. DESMARETS has further

 obſerved,

obſerved, that the priſmatic *baſaltes* of Auvergne is actually a continuation, and generally the termination, of a current of *lava*.

3. Although the variety of the forms of the cryſtals, in the ſame kinds of glaſs, and even in the ſame piece of glaſs, which has been already remarked, ſufficiently ſhews the uncertainty of any inference drawn from a ſimilarity of ſhape; yet it may not be improper to mark the analogy, in this reſpect, between the baſaltic and vitreous cryſtals. The columnar or priſmatic form is known to appear moſt generally in the cryſtallized *baſaltes*. Of this form alſo are evidently the cryſtals repreſented by fig. 5. The ſemi-columnar, vitreous cryſtals, fig. 9. ſeem to be analogous to the no leſs ſingular baſaltic ſemi-columns obſerved in the Giant's Cauſeway by Bp. POCOCK (Phil. Tranſ. vol. XLVIII.); which, he ſays, were exactly like hexagonal columns cut in two. M. DESMARETS has obſerved, in the province of Auvergne, great quantities of ſpherical and ellipſoid baſaltics concretions, which were formed of polygonal columns, rather pyramidal than priſmatic, converging from the circumference to the center. Theſe ſeem to be perfectly analogous to the vitreous globular concretions which have been above obſerved to be compoſed of oblong cryſtals, arranged in a ſimilar manner. The ſame author alſo obſerved, in the ſame province, regularly-ſhaped tables or plates of baſaltes; of which, he ſays, aſſemblages were accumulated in all directions. We have ſhewn, that the cryſtals repreſented by fig. 1. 2. 3. and 4. are

are really aſſemblages of plates or tables, diſpoſed in every direction round a common axis.

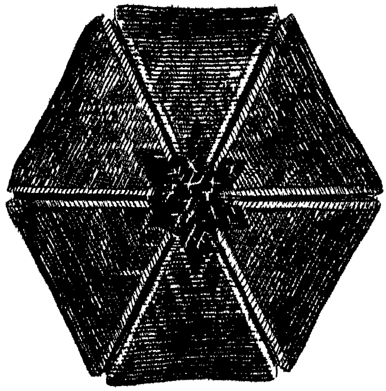
Laſtly, The ſtone on which the columns of *baſaltes* generally reſts, and which ſometimes alſo is ſupported by theſe columns, being of the ſame nature and texture as the columns themſelves, ſeems to be a maſs irregularly cryſtallized, analogous to the irregularly-ſhaped maſſes in the ſpecimens of glaſs N° 1. and 2. which evidently conſiſt of a ſimilar ſubſtance as the neighbouring cryſtals, and ſeem to have been compoſed of a number of theſe cryſtals indiſtinctly united; for the peculiar figures of cryſtals are diſtinct only when they are inſulated, or when they are ſeparated from each other by a pellucid or differently-coloured medium. A medium of this kind appears between the vitreous cryſtals, and is nothing elſe than the more fluid parts of the glaſs, which longer reſiſt the concretion, but which, by a further continuance of the heat, would have become, with the parts already cryſtallized, one uniform, white, opaque ſubſtance, without any interpoſition of tranſparent glaſs, or diſtinction of cryſtal, except on the ſurface, as in ſpecimen N° 5, in which the cryſtals ſtand prominent from the indiſtinct maſs, and unenveloped in any medium, in the ſame manner as the baſaltic cryſtallizations appear ſtanding above the maſs of ſtone or *lava* which ſupports them.

Further obſervations on the baſaltic and vitreous cryſtals may probably ſuggeſt more inſtances of analogy

 between

between theſe two ſubſtances. No juſt objection can be drawn againſt this analogy from the magnitude of the former compared with the minuteneſs of the latter: for the difference of ſize between the ſmall vitreous cryſtals and the ſtupendous baſaltic columns, which ſupport mountains, iſlands, and provinces, is no more than is proportionate to the difference uſually obſerved between the little works of art and the magnificent operations of nature.

Fig 2



Fig

Fig 9

Fig 7

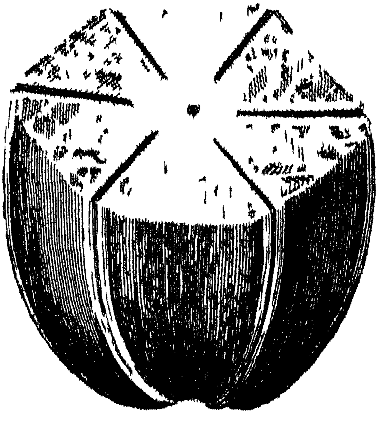


Fig 3

Fig 8

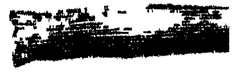
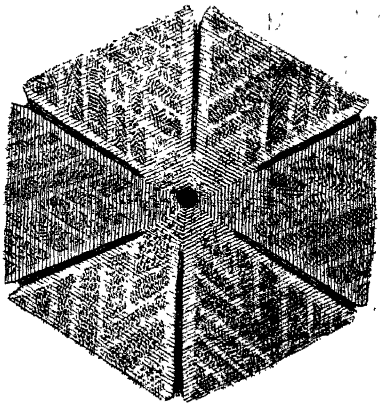


Fig 5

Fig. 4.



...g. 6.

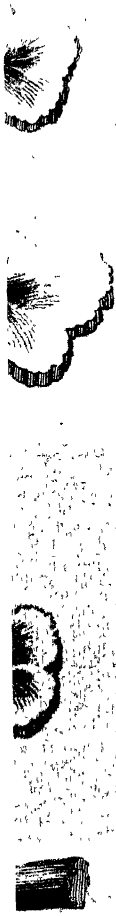
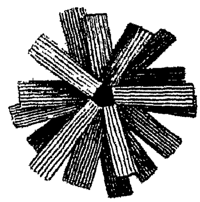


Fig. 10.



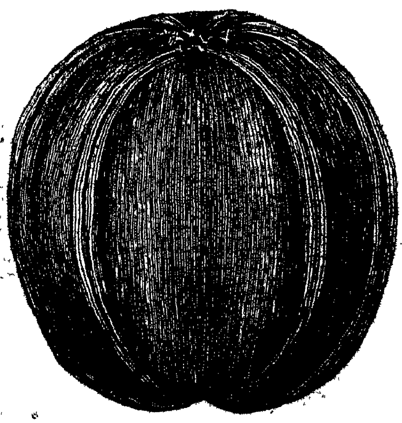


Fig. 1.

Basire sc.

XXXV. *A Belt on the Diſc of* Saturn *deſcribed in an extract of a Letter from Mr.* Meſſier, *F. R. S. to Mr.* Magellan, *F R. S. Dated* Paris, May 29, 1776.

R. June 6, 1776. I HAVE obſerved, ſince the 14th of May, a belt of a fainter light on the body of Saturn, oppoſite to the part of the ring behind the planet. It is pretty broad, and almoſt as diſtinct as thoſe of Jupiter. It was with a very good achromatic of three foot and a half, made by Mr. DOLLAND, that I diſcovered this appearance. I wiſh you would communicate it to the aſtronomers, becauſe thoſe who are furniſhed with better inſtruments may, perhaps, ſee ſome inequalities in this belt of Saturn, and ſo the time of the planet's revolution on its axis may be better aſcertained than it is at preſent. Meſſ. JOHN and JAMES CASSINI ſeem to have been the only aſtronomers who diſcovered this phenomenon about the end of the laſt century.

XXXVI. *An Account of ſome poiſonous Fiſh in* The South Seas. *In a Letter to Sir* John Pringle, *Bart. P. R. S. from Mr.* William Anderſon, *late Surgeon's Mate on board His Majeſty's Ship the* Reſolution, *now Surgeon of that Ship.*

TO SIR JOHN PRINGLE, BART. P. R. S.

Reſolution,
Deptford, April 23, 1776.

S I R,

R. June 6, 1776.

IN compliance with your requeſt I have ſent you the few notes which I had taken of the caſes of ſome of our ſhip's company, who, on our late voyage to the South Sea, had experienced the bad effects of eating certain fiſh of a poiſonous nature. I was, perhaps, leſs ſolicitous about remarking the minute circumſtances attending their illneſs, as I then believed it was a diſorder well known in the Weſt Indies, having frequently heard of people being poiſoned, as it is commonly expreſſed, by eating ſome particular kinds of fiſh; but, as far as I have been able to inquire ſince of thoſe who have been there, or from books, I do not find that any tolerable account either of the diſeaſe, or of the means of curing it, has been made public.

This being the caſe, it is almoſt needleſs to ſay, that in treating the diſorder we could have no method founded

on

on experience to purſue, and therefore were obliged to palliate the ſymptoms, from the analogy they bore to thoſe that occur in other diſeaſes. From a ſuppoſition, that ſome of the matter, which cauſed the illneſs, might lie indigeſted in the ſtomach, Mr. PATTEN, the ſurgeon, ordered ſome warm water to be drunk, in order to make the patients vomit; which effect it had with ſome of them, and they were a little relieved by it. After the *nauſea* had ceaſed, he gave ſome weak portable ſoup, as a diluent; and for the moſt troubleſome ſymptom, *viz.* the heat on the ſurface of the body, he preſcribed a ſudorific julep, whereof the active ingredients were the antimonial wine and *ſpiritus mindereri*. This, in ſome meaſure, had the deſired effect, as it brought on a breathing ſweat, which, for the time it continued, abated the violence of the pains. No other medicines were uſed, excepting ſome purging ſalts, for preventing inflammation, in two or three, whoſe mouths and throats had been more particularly affected. Their diet conſiſted chiefly of tea, ſago, and portable ſoup.

I have avoided ſaying any thing about the manner in which the poiſon operates, as the inſtances have been too few, to draw any certain conſequences from them. I would only obſerve, that its action may be ſuch, as to affect and deprave ſome of the organs of ſenſation, without much irritating the firſt paſſages; becauſe in all the patients the diſorder of the ſtomach and bowels had long ceaſed before the other ſymptoms went off. And I was confirmed in this opinion by a circumſtance which afterwards

afterwards happened to Captain COOK, who, having eaten a ſmall piece of the liver of another kind of fiſh (a *tetraodon)* was not ſenſible of being hurt by it, till waking in the night, and calling for a draught of water, he neither could feel the veſſel with his hands, nor was ſenſible of its weight when he graſped it. On the other hand, it was remarked, that ſome of the other gentlemen, who had likewiſe eaten of that fiſh, had alſo a vomiting and looſeneſs. The difference, perhaps, depended on the quantity taken into the ſtomach, and the particular conſtitution of the perſon.

I ſhall only add here, that, having been favoured by Mr. BANKS with a ſight of his drawings, I find the *Sparus Pagrus* of LINNÆUS to be the fiſh which that eaten by our people moſt reſembles. It is probable, that it is the ſame fiſh that QUIROS found to be of a poiſonous nature; but at the ſame time I muſt obſerve, that it may well be doubted, whether this ſpecies is always poiſonous, as our men ate another of the ſame ſort about a month after, without being affected by it. I am, &c.

SATURDAY, July 23, 1774, on board His Majeſty's ſhip the Reſolution, off the Iſland Malicolo, in The South Sea, three fiſh, of the ſame ſpecies, that had been caught, being dreſſed for dinner, affected all thoſe, who ate of them, in an uncommon manner; but five perſons, who had eaten of one of them, were more ſeverely attacked than the reſt. Immediately after eating, nothing was felt but ſome uneaſineſs (or ſuch pain as follows

from swallowing any acrid substance) in the mouth and throat. About two in the afternoon, some felt an uneasiness in the stomach, with an inclination to vomit; but it was near the evening before those who suffered most were affected. The symptoms at first were universal lassitude and weakness, followed by a retching; and in some, by gripings and looseness. To these succeeded a flushing heat and violent pains in the face and head, with a giddiness and increase of weakness; also a pain, or, as they expressed it, a burning heat in the mouth and throat. Some had the mouth affected in such a manner, that they imagined their teeth were loose; which might really be the case, as a considerable spitting attended this symptom. The pulse all this time was rather slow and low. The retching and looseness did not last long; but the pain and heat of the head were extended to the arms, hands, and legs. The patients continued in this manner all the night, but with some intervals of ease. Towards the morning, the pains, especially those in the legs and arms, but more particularly about the knees, were severer than before. These would sometimes remit and frequently shift, or be more violent in one place than in another. Sometimes the pain would remove suddenly from the legs, and fix in the head; the palms of the hands were hot; and the fingers, legs, and toes, felt often as if benumbed: nay, the whole limbs became in some measure paralytic, the sick person being unable to walk unless supported. Although there appeared no swelling in the face, it might be observed

to have a ſort of ſhining or gloſs upon it; and the patient ſometimes imagined his noſe was grown to a great ſize.

24th, In the forenoon they continued much the ſame; but ſome, after ſleeping, were rather eaſier; and one had a copious ſpitting, which, however, gave him no relief, for at noon the pains in his limbs ceaſing were removed to his head, which they affected violently with a ſenſe of throbbing and great heat; nor were there any of the patients, but this one, who complained much of thirſt during the illneſs.

Another, in particular, had the pains in his knees ſo increaſed, that they made him cry out. The uneaſineſs at the ſtomach and heat of the throat in all had nearly ceaſed. When the *mucus* about the *fauces* was forced away by ſtraining, it felt hot, and left the ſame ſenſation about the throat for ſome time. In the afternoon, moſt of them grew much eaſier, but continued weak; and two, who now ſeemed better than they had been before, complained of heat and ſoreneſs in their hands and feet.

25th, All five had reſted tolerably in the night; but complained of weakneſs and ſoreneſs of the mouth, with heat in the hands and feet. One, who had been rather worſe than the others, ſtill had a conſiderable ſpitting.

26th, All continued better, but the pains were not entirely gone. The great weakneſs, with heat in the hands and feet, were ſtill general complaints, and the ſoreneſs in the mouth remained in ſome. The one mentioned as having a large diſcharge of *ſaliva*, continued ſpitting;

spitting; and another began to have the same complaint, though in a less degree.

27th, One of the five had no other complaint but a disagreeable sensation on rubbing his skin in any part of his body. Another, as yesterday, mentioned a slight giddiness, weakness, &c. A third, who had been the worst of all, and who had taken some purging salts, was much better. The other two, who had seemed so well the day before, became worse at night; one of them, who before had told me of violent pains in his knees, had a return of the same symptom; the other had pains in his legs, and an universal uneasiness.

28th, They were all considerably better, except one, who had been seemingly well the night before, but complained now of much weakness, and flying pains in his limbs.

29th, All of them mended, but still complained of wandering pains in the limbs, weakness and heat in their hands and feet.

30th, Continued as yesterday. At times they seemed quite well; but, in the morning, they complained of more weakness than in the preceding evening; and the pains, which generally appeared to be almost gone in the day, returned at night.

31st, All were better, but not without some slight pains and a weakness in the morning; and some still felt a disagreeable heat in their hands and feet, and others in their mouth.

August 1, One had no complaint. The reſt not altogether free from pains, and a ſenſe of wearineſs or weakneſs.

2d, All were pretty free from pain; but one had ſtill too much heat in his hands.

3d, All recovered; ſome trifling pains and a little weakneſs excepted.

Theſe notes I kept of the five who had eaten of one fiſh. With regard to thoſe who had eaten of the other two fiſh, they were not ſo ſeverely handled. No ſigns of illneſs appeared amongſt them till night; ſome had then a *nauſea*, retching, and ſome looſe ſtools. Others felt only pains in the backs, arms, and legs, as in a rheumatiſm; but it was obſerved, that in all, the face was more or leſs affected with an uneaſy ſenſe of ſtiffneſs. All theſe, though at firſt ſlightly ailing, and though the ſymptoms were later in coming on, had this circumſtance attending them, that they were all worſe next day in the evening.

On the 25th, they continued much the ſame as on the day before in the afternoon; but, towards night, they grew ſomewhat better. They had pains in their arms and legs, with a weakneſs and ſenſe of heat on the ſurface of the body.

26th, All complained in the ſame manner; and, though the pains were leſs, they continued, often ſhifting ſuddenly from one place to another, and the weakneſs rather increaſed.

27th, Two of them, who had ſuffered leaſt, remained much the ſame. The others were ſenſibly better.

28th, Moſt of them had wandering pains in their arms and legs, but were, in other reſpects, viſibly better. One who, for the firſt two or three days, had ſcarcely complained, ſaid, that this day he had a head-ach and pains in his legs and arms.

29th, None of them yet perfectly recovered; all complaining of leſſer pains in their legs, arms, or back.

30th, All continue much the ſame.

31ſt, All better, but not altogether free from pains and weakneſs.

Aug. 1ſt, Some were recovered, and the reſt better.

2d, All of them were pretty well; and they continued ſo.

Several of our dogs, who had eaten of theſe fiſh, and eſpecially of the guts and bones, ſeemed affected in a higher degree than the men. Their illneſs might, perhaps, have begun in the night, but was not perceived till next morning. One, in particular, had violent retchings, a *paralyſis* (eſpecially of the hind legs), lay down, rolled, howled, and ſhewed other ſigns of great pain. More *ſaliva*, or froth, hung about his mouth; and, when fatigued with ſtruggling, would fall down, and ſeemingly breathe with great labour. Two of them continued in the ſame way all day; though others that had eaten as much appeared but ſlightly affected, their principal ſymptom being only weakneſs in the hind legs. It was

obſerved, that moſt of them were troubled with a ſwelling of the *penis*.

One lay the firſt day, all the afternoon, without being able to move, but groaned perpetually, and lay in appearance in great anguiſh.

Next day, they ſeemed all ſomewhat better, except one, to whom tobacco-juice had been given the day before, to make him vomit: he died in the afternoon.

On the third day, though the dogs were more free from pain, yet they continued almoſt motionleſs, nor did they begin to eat.

On the fourth day, they were all much better, the worſt of them running about, and eating his victuals.

On the fifth, all of them ſeemed recovered, one excepted, who had not been ſo ſoon or ſo violently attacked as the others. This remark was made of the men likewiſe; to wit, that thoſe who were more ſlightly and later attacked continued ill as long as the others. Some days after, the ſame dog became ſo paralytic in his hind legs, that he could not ſtand; but afterwards he recovered the uſe of them pretty well.

A hog, who had eaten of the offals, died next morning; as did a perroquet on the ſecond day, which had got from his maſter ſome of the boiled fiſh. It will be proper to add, that another hog died, which had fed on the entrails of that fiſh whereof Captain COOK had eaten a bit of the liver; and that a dog, which about three weeks after had eaten of a fiſh of the ſame ſpecies, died, after a lingering illneſs, of the ſame nature with that which had affected others of his kind. XXXVIII. *Ex-*

XXXVIII. *Experiments on Ignited Subftances. By Mr.* John Whitehurft, *in a Letter to* James Stuart, *Efquire, F. R. S.*

TO JAMES STUART, ESQ.

S I R, London, Nov. 29, 1775.

R. May 23, 1776. THE experiments of Mr. BUFFON upon ignited bodies feem to prove, that, when heated to the degree he mentions, they are more ponderous than when cold. The experiments which I have made on heated metals, fuggeft a different idea, and contradict the fact he relates; fo that I am induced to believe, that fome circumftance, not attended to, has introduced a miftake in the relation this learned philofopher has publifhed as the refult of his inquiry.

His experiment ftands thus recorded (Suppl. Nat. Hift. vol. II. p. 11.): a mafs of iron, after receiving a white heat, weighed 49 lbs. 9 oz.; when reftored again to the temperature of the atmofphere 49 lbs. 7 oz. Hence he concluded, that the igneous particles, contained in the heated iron, increafed its abfolute weight 2 ounces.

My experiments are as follows: 1ſt, One pennyweight of gold, made red-hot, became apparently lighter; but, when reſtored again to the temperature of the atmoſphere, its former weight was perfectly reſtored.

2d, One pennyweight of iron, heated as above, was alſo apparently lighter; but, when it became cool again, its weight was viſibly augmented.

It is now ſeveral years ſince I made theſe experiments; but I well remember to have repeated them ſeveral times, and that the reſults were always the ſame. It may be neceſſary to remark, that the beam uſed in theſe experiments was ſenſibly affected by the $\frac{1}{2000}$th part of a grain; and likewiſe, that each of the metals was heated upon charcoal, by means of a candle and a blow-pipe, and both were brought nearly to a ſtate of fuſion.

It ſeems needleſs to obſerve, that the apparent levity of the gold and of the iron, when hot, was owing to the aſcent of the rarified air above the ſcale, and to the tendency of that underneath to reſtore the equilibrium of its preſſure. The increaſe of weight in the iron might probably ariſe from its having, in ſome degree, acquired the property of ſteel, by means of the flame and charcoal.

I am at a loſs to account for the fallacy which ſeems to have attended M. BUFFON'S experiment; but it ſeems probable, that the heat of the maſs of iron employed by him, had a greater effect on that arm of the beam from which

which it hung than on the other, which being lefs heated, would confequently be lefs expanded; and this difference of expanfion might produce the error in M. BUFFON's account of the weight of heated iron.

If thefe obfervations afford you any fatisfaction, it will give me pleafure.

I am, &c.

XXXIX. *An Account of a Suppression of Urine cured by a Puncture made in the Bladder through the* Anus; *being an Extract of a Letter from Dr.* Robert Hamilton, *Fellow of the Royal College of Physicians at* Edinburgh, *and Physician at* King's-Lynn, *in* Norfolk, *to Sir* John Pringle, *Bart. P. R. S.*

R. May 23, 1776.

I WAS sent for on the 25th of March, 1774, to visit JAMES WILKINSON, of about thirty-one years of age, the son of a farrier in this town, who had laboured under a suppression of urine for three days.

The account this man gave of himself was, that he had been fourteen years a soldier in a regiment of foot, but had been discharged the preceding year as unfit for service, on account of this disorder, to which he had been long subject, and which was supposed to originate from a stone in the neck of his bladder; that, about twelve years before his discharge, he had contracted a *gonorrhœa*, which had been badly cured; that, from that period, he had become afflicted with frequent suppressions of urine, and that every return had been of longer duration than the former; that when the suppression had continued some time, he had been used to go to the Regimental Hospital, and to stay till it was removed; and that, bleeding,

bleeding, purging, and the warm-bath had been the means uſed for his relief. He added, that the catheter could never be introduced into his bladder from the firſt attack; that the firſt ſigns of relief were a few involuntary drops of urine iſſuing from the *urethra*, after which followed a ſmall thread-like ſtream, which continued to run until the bladder was a little more than half emptied; for he did not think that his urine had at any time been perfectly diſcharged, during the thirteen years he had been afflicted with this painful diſorder. He concluded with telling me, that in other reſpects he had enjoyed good health; and that the preſent ſuppreſſion had been brought on by hard riding on a journey he had undertaken to go expreſs.

This man was in a very diſtreſſed condition. His bladder was diſtended to an enormous ſize, reſembling the gravid *uterus* at that late period of geſtation, when the *fundus* reaches above the navel; to ſuch a degree was it dilated by the repeated ſuppreſſions of urine for ſo many years. His pulſe was ſmall and quick; he had been troubled with a hiccup for many hours, and had vomited every thing he had taken from the firſt day of his illneſs. His head and face were emphyſematous from ſtraining; and the cellular membrane was ſo much inflated, that he could not open his eye-lids. He was attended by two ſurgeons, who had uſed bleeding, opiates, clyſters, fomentations, and the warm-bath, without any benefit; and they had repeatedly attempted to introduce catheters of different ſizes, without ſucceſs; that inſtrument, having paſſed without reſiſtance to the neck

 of

of the bladder, met there with an obftruction not to be overcome. Under fuch preffing circumftances no time was to be loft. The patient's life was in imminent danger, either from a rupture, or a gangrene of the bladder. The approach of the latter was foon to be apprehended, from the hiccup and fmallnefs of the pulfe. There was no alternative left; the bladder muft be fpeedily emptied by an aperture fomewhere, or the patient muft perifh. The furgeons, therefore, went to prepare their inftruments for making an opening into the bladder. Reflecting, in their abfence, on the moft eligible method of performing the operation, I was in fome degree of perplexity; for I did not much approve of any of the methods recommended by the beft chirurgical writers; either that of making a perforation above the *os pubis*, in the place where the higher operation of lithotomy is performed; or that of the puncture *in perinæo*. Whilft I was confidering what was to be done, the mother of the patient told me, that fhe had feveral times that day attempted to give him a clyfter, but had not been able to introduce the pipe, by reafon of a large fubftance low in the gut near the fundament, which ftopped up the paffage. It immediately occurred to me, that the obftructing body could be no other than the diftended bladder, which, having filled the *pelvis*, preffed downwards where there was the leaft refiftance towards the *anus*, as well as upwards into the *abdomen*. From this circumftance I was led to think of difcharging the urine by a puncture into the bladder, with a trocar introduced by the *anus*. I conceived that this method would have advantages fuperior

to

to any other that I had heard of, for ſimplicity, eaſe, and ſafety. The finger could guide the point of the inſtrument to the very ſpot of the bladder to be pierced. The coats of the *inteſtinum rectum* and bladder, and the intervening cellular membrane, were all that were to be perforated, and they were now preſſed ſo cloſely together, that there could be no more art required than in piercing any ſimple bag of water. When the ſurgeons returned, I told them what had occurred to me concerning the deſcent of the diſtended bladder (for I had not yet examined it) and its preſſing the *rectum* downwards; I repreſented to them the hazard and difficulty attending the operation hitherto in uſe, and propoſed this method of perforating the bladder with a trocar introduced by the *anus*. They readily acknowledged the advantage of ſuch a practice, and agreed to give it the preference. We examined with a finger *in ano*, and felt a large round tumor, a very little way within the orifice, preſſing the anterior ſide of the *rectum* downwards, and puſhing the *anus* and *perinæum* conſiderably outwards. The gut itſelf was looſe and empty; and through its relaxed ſides the tumor, which was evidently the bladder, was diſtinctly felt ſtretching every way, completely filling the *pelvis*, and feeling like the membranes which contain the waters of a woman in labour, thruſt into the dilated *vagina*. I deſcribed the manner in which I thought the puncture ſhould be made; and, as I imagined that I could better execute what I had myſelf conceived than another perſon, I offered to do it; which being readily aſſented to,

 the

the operation was performed in the following manner. Having placed the patient on his back on a bed, with his breech projecting a little over the side of it towards the light, and his legs bent into the position they are placed in for the operation of lithotomy, and held by two assistants, a trocar of the middle size, with its point guarded by the extremity of the fore-finger well oiled, was introduced into the *anus*, until the tip of the finger reached the anterior part of the tumor; when the finger being a little withdrawn, and the point of the instrument brought into contact with the tumor, it was plunged into it, in a direction parallel to the axis of the bladder, in an erect posture; and the perforator being pulled out, the water immediately followed. A straight catheter was quickly introduced through the canula into the bladder, lest, as it collapsed and shrunk upwards as the water was discharged, the canula should prove too short, for its shell was then close to the *anus*. The canula was then slipped out of the aperture as far as the rings of the catheter, as being no longer of use, and the catheter remained in the bladder until the urine was all drawn off; during which time, that instrument was moved different ways, to search for the stone which was supposed originally to have occasioned the disorder; but none was to be found. The water being discharged, the catheter was taken out, and the patient put to bed. The parts were repeatedly fomented; and a draught, with half a drachm of nitre and 25 drops of laudanum were given, and two more of the same kind were ordered for the night.

26th,

26th, He had had a very good night, and had made water five or six times through the aperture made by the trocar. He said, that as soon as the bladder had collected a certain quantity of urine, he had felt an inclination to make water; that then sitting on a chamber-pot or bed-pan, and straining in the usual way, the urine had rushed out at the aperture *per anum* in a stream; and that none had passed by the *urethra*. Not contented with his account of the matter, I desired him to make water in my presence, and was witness to this curious and extraordinary power of retention of the urine in a wounded bladder, and of discharging it at pleasure through an artificial passage. The emphysematous swelling of his head and face was almost gone. He was directed to drink the pectoral decoction, with the addition of some marsh-mallow root, sweetened with manna, and acidulated with orange or lemon-juice; and the nitrous opiate was repeated.

27th, He complained of a fulness of his belly, probably owing to his not having had a stool. He still made water through the trocar-aperture as before, and whenever he pleased; but now he began to perceive a little urine come by the *urethra* at the same time. We did not choose to order a clyster, lest the gut should be injured by the pipe, but to wait the effect of the manna, which, with the night-draught, was still continued. The resolution of the inflammation being now begun, as was conjectured by the urine finding its natural passage by the *urethra*, a bougie was introduced beyond the stricture at the neck of the bladder, and to very good purpose.

28th,

28th, He was much better, he had had ſome ſtools, and had paſſed moſt of his urine by the *urethra*.

30th, No water had iſſued through the aperture either this morning or the preceding day; the whole, though in a ſmall ſtream, had flowed by the *urethra*. He complained of a ſoreneſs *in ano*.

31ſt, This day he felt that ſoreneſs only when he went to ſtool. The ſtream of urine by the *urethra* was ſtill ſmall.

April 6th, The puncture through the *rectum* and bladder appeared to be quite healed. The urine was diſcharged in a tolerable ſtream, the paſſage being, as he obſerved, much wider than it had been for thirteen years before.

He continued the daily uſe of the bougies; and, being ſenſible of the great benefit he had received from them, willingly perſevered in their uſe, until the ſtricture was ſo much leſſened, as to permit a free diſcharge of his water, and by theſe means he obtained a complete cure: for, in two months after, he left the town in every reſpect well. It was remarkable, that, during the progreſs of the cure, no urine was perceived to ooze involuntarily through the opening; but it was always retained until the patient, prompted by a fulneſs of the bladder, made his water, as has been related.

Having given as accurate an account of this caſe as I am able, I muſt now, SIR, in juſtice to myſelf, beg leave to aſſure you, that I had neither heard, nor read, of any method of perforating the bladder, ſimilar to that which I have related, before you kindly informed me in your

laſt letter, that it was not a new operation; and that I ſhould find an account of it in POUTEAU'S *Melanges de Chirurgie.* I have procured the book, and have read the paper with great ſatisfaction, and am much obliged to you for your intelligence. I do not preſume to claim any merit from a thing which took its riſe from mere accident. The obſtruction which the pipe met with when the patient's mother attempted to give him the clyſter, ſuggeſted the method of piercing the bladder by the *anus*; and I am perſwaded, that the ſame thought would have ariſen, on a like occaſion, in the mind of any thinking man. And I beg you would believe, that I have no ſmall ſatisfaction in finding it occurred to M. FLURANT, the ingenious author of the paper on that ſubject, in that publication, from a circumſtance, which, though not exactly ſimilar, amounted nearly to the ſame thing. This ſurgeon having introduced his finger into the *anus*, to examine the ſtate of the bladder, in order to perform the puncture *in perinæo*, found it ſo round and tumid, and ſo much within the reach of his inſtrument, that he thought he could perforate it with ſafety in that place; and, from a little reflection on the ſtructure of the parts, was convinced of the expediency of operating in this manner, in preference to any other.

Before I quit this ſubject, I think it proper to acquaint you, that I have found a compoſition of calomel and opium, in large doſes, the beſt internal remedy for ſuppreſſions of urine, in general; and that I have repeatedly ſeen this medicine ſucceed after the uſual means have failed. I am convinced,

convinced, from theſe trials, that the principal or ſpecific efficacy is in the calomel, as large doſes of opium alone, or joined with camphire, have proved unſucceſsful. I am ſo well ſatisfied of the advantages of this practice, that, if called early in the diſorder, I direct ten grains of calomel with two grains of ſolid opium, made into a bolus with any conſerve, to be taken immediately, and repeated in ſix hours. I have ſeldom occaſion to order a third doſe, the patient being generally relieved by the ſecond, if the firſt has failed. I did not adminiſter it in the caſe here related, becauſe the alarming ſituation the patient was in when I came to him required the bladder to be emptied without delay.

XL. *Obſervations made during the late Froſt at* Northampton. *By* A. Fothergill, *M. D. Communicated by* William Henley, *F. R. S.*

TO THE REV. DR. HORSLEY, SEC. R. S.

REV. SIR,

R. June 27, 1776.

THE following letter, from my learned friend Dr. FOTHERGILL, contains, in my opinion, many very curious experiments and obſervations; and, though it was not intended for that purpoſe, I cannot but think it well deſerving the notice of the Royal Society. I have, therefore, SIR, taken the firſt opportunity to put it into your hands.

And am,

Yours very ſincerely,

W. HENLY.

DEAR SIR, Northampton, May 3, 1776.

ACCORDING to my promiſe, I now proceed to give you a ſhort account of ſome obſervations and experiments concerning the late ſevere froſt. As ſome of the phenomena appeared to me not a little ſurprizing, I minuted them down at the time they occurred.

Jan. 27th, The great quantity of ſnow which had continued falling almoſt every day for three weeks, had, for theſe five or ſix days paſt, rendered the roads impaſſable; and the poſt, both upwards and downwards, was ſtopped, the ſnow being drifted from ſix to ten feet deep or upwards. This morning the froſt became ſuddenly very ſevere; the wind full Eaſt, accompanied with ſnow. The barometer ſtood at $29\frac{3}{4}$. A thermometer, according to FAHRENHEIT'S ſcale, which hung in my parlour, where there was a good fire, ſtood at 33°, that is, only 1° above the freezing point. After it had been ſuſpended a quarter of an hour on the Chineſe paliſades before the ſtreet-door facing the South, it ſunk to 20°, that is, 12° below the freezing point. At five o'clock the ſame evening, it fell to 16°. At this time eggs in the market cracked in the womens baſkets, and appeared in a coagulated ſtate, of the conſiſtence of bees-wax. This evening was placed on my garden-wall, facing the Eaſt, half an ounce of each of the following liquors in a cup; *viz.* lemon-juice, vinegar, and red port-wine.

Jan. 28th, This morning, at eight o'clock, the barometer ſtood at 30. The thermometer at 12°, that is, 20° below

below the point of freezing; wind Eaſterly; the atmoſphere clear and ſerene, but piercing cold. The three liquors were reduced to a ſolid cake of ice. This night, about eleven, were placed on the ſame wall the following liquors; *viz.* ſpirit of *Mindererus*, volatile ſpirit of *ſal ammoniac* of both kinds, mild and cauſtic, dulcified ſpirit of nitre, red port-wine, and French brandy.

Jan. 29th, Barometer $29\frac{9}{10}$; thermometer at 11°, that is, 21° below freezing; the Eaſterly wind exceſſively keen and piercing. The roads which, at great labour and expence, had juſt been cut through for carriages to paſs, were again this morning, though no freſh ſnow had fallen, compleatly drifted up. Theſe liquors alſo, to my great ſurprize, now ſhewed evident marks of freezing. They were ſuffered to remain, and two more cups were placed near them, with highly rectified ſpirit of wine and vitriolic ether. At a little diſtance was placed, in a frigorific mixture, conſiſting of a combination of the vitriolic acid with ſnow, about an ounce of crude quickſilver in a phial.

Jan. 30th, The morning clear, but intenſely cold; wind S.E.; barometer $30\frac{1}{10}$; thermometer ſunk to 9°, that is, 23° below the freezing point; a degree of cold which, I apprehend, has been but rarely experienced in this climate, being $3°\frac{1}{2}$ below that of the remarkable froſt in the year 1739. On examining the liquors on the garden-wall I found, to my aſtoniſhment, all of them, except the ſpirit of wine and ether, perfectly congealed: the firſt time I had ever ſeen theſe liquors in a ſolid form.

Being deſirous to ſee the effect of a high degree of artificial, added to the natural cold that now prevailed, the thermometer was immerſed into the frigorific mixture; but though it ſunk the quickſilver, in a few ſeconds, into the bulb of the thermometer, yet the reſult was by no means adequate to that of the experiment of Profeſſor BRAUN at Peterſburg: for although the quickſilver in the thermometer, and that in the phial, contracted a film on the top, yet it remained quite fluid below.

Jan. 31ſt to Feb. 1ſt, The barometer at 29; the thermometer only at 16°, that is, 17° below the point of congelation; the atmoſphere ſerene and pleaſant.

Feb. 2d, Wind S.; barometer $29\frac{1}{2}$; a warm, miſty morning, ſucceeded by a pleaſant, ſpring-like day, uſhered in a very mild and agreeable thaw, the thermometer from 9° being got up to 40°; ſo great was the change of temperature in ſo ſhort a ſpace of time! And it ſeems worthy of obſervation, that the epidemic cold, which had prevailed univerſally during the preceding mild ſeaſon, ſuddenly diſappeared in the late intenſe froſt; but now began to re-appear, together with rheumatic affections and other diſeaſes of the former period.

I am, &c.

XLI. *An Account of the Magnetical Machine contrived by the late Dr.* Gowin Knight, *F. R. S. and presented to* The Royal Society, *by* John Fothergill, *M. D. F. R. S.*

TO THE PRESIDENT AND FELLOWS OF
THE ROYAL SOCIETY.

GENTLEMEN,

R. June 27, 1776.

BY being left executor to your late worthy member GOWEN KNIGHT, M. B. a very extraordinary magnetic machine of his contrivance, and which had coſt him much labour and expence, came into my poſſeſſion. This, I thought, might not be unworthy of a place in your repoſitory; and I therefore, deſire your acceptance of it, as a monument of Dr. KNIGHT's very ſingular abilities, and of my regard to the purpoſes of your inſtitution.

I muſt, however, inform you, that this machine, which, by the annexed figure and its explanation, may be obſerved to conſiſt of two parts, is by no means ſo ſtrongly magnetical as it was at the doctor's deceaſe. Not long after this event, it was neceſſary to remove this apparatus from his

his apartments in the Britiſh Muſeum. One of theſe parts was fixed up in your Muſeum, the other was left at the lodgings of one of your very uſeful, ingenious members, J. H. DE MAGELLAN, for the purpoſe of ſome experiments, and alſo for impregnating ſtrongly the needles of ſea-compaſſes. Here it was accidentally deſtroyed by fire, and the parts it conſiſted of rendered almoſt wholly uſeleſs. A new one has, however, been made, and impregnated with the magnetical power, by the ingenious gentleman abovementioned, according to the method of Dr. KNIGHT. It has acquired a conſiderable degree of magnetic force, by being placed in the polar line with the other part of this machine that was unhurt, and where in time it will, perhaps, acquire a conſiderable degree of magnetic energy.

I wiſh it had been in my power to have given a minute and pertinent detail of my deceaſed friend's diſcoveries in this branch of knowledge. He acquainted me, it is true, at different times in converſation, of the progreſs he had made in theſe diſcoveries; but, as I then thought he intended to leave behind him an exact account of his experiments and reſearches and their reſult, I rather liſtened to his relations as matters of inſtructive amuſement, not thinking it would ever be neceſſary for any other perſon than himſelf to give the public an account of his labours. Indeed, there are many uſeful memorials of his, on this ſubject, in your own collections, to which I muſt refer the inquiſitive reader. I ſhall only mention

ſome

ſome circumſtances, relative to this machine, which I do not know have been related either by himſelf or any other perſon.

The firſt thing, I believe, that engaged the doctor's attention more particularly to magnetiſm, was the accident that befel a ſhip's compaſs from lightning; and of which, I think, the doctor gave a very circumſtantial account to the ſociety. This affair led him to conſider the ſtructure of the compaſs more minutely. He procured compaſs-cards ready-armed, as it is called, from different makers both at home and abroad. He found moſt of the needles ſtrangely erring from due polarity; ſome being many points to the Weſt, others as many to the Eaſt, of the right poſition. Amongſt them all there was only one, which to him ſeemed conſtructed on a rational plan, and was of French make, procured from Marſeilles; but even this was not without very evident faults.

To fix upon the proper form of a needle through which the magnetic effluvia could paſs with the leaſt interruption, to give the needle ſuch a degree of hardneſs as to retain the magnetic influx the longeſt, and with the greateſt force, were material objects; and, I imagine, a view to have ſuch a degree of magnetic power at his command, as to force the magnetic virtue through the moſt conſolidated bars, was his firſt inducement to try, whether he could not collect ſuch a magazine of magnetiſm, as would be ſufficient for every purpoſe of this kind, and at the ſame time exhibit ſome new phenomena in

phyſics yet undiſcovered. With this view he planned and executed the machine, deſcribed at the end of this relation.

His firſt attempt, however, was much ſmaller; a few bars were laid in the due courſe of the magnetic flux, and impregnated by conſtant attrition. To theſe other bars were added ſucceſſively, after they had been impregnated, both by the force he could give them by attrition, and what he could derive from the preceding ſtock collected in the bars. To theſe he added ſtill freſh bars, till he had formed the whole maſs as it is now preſented to you, and reſting on wheels and pivots, in ſuch manner as to be eaſily manageable for the purpoſe of impregnating the needles he was employed to ſee prepared, for the ſervice of government, and others, who had generoſity enough to think, that the compaſs, on which depended the lives of the ſhip's crew, could not be made too perfect, and that it deſerved a reaſonable compenſation. It is to the doctor's ingenuity and indefatigable attention to this uſeful inſtrument, that it has acquired amongſt us a degree of perfection unknown to our predeceſſors.

When the machine was compleated, he ſtill was adding continually to its power. He impregnated every ſingle bar of which it is compoſed, by repeated attritions, and applied it to the remaining bars in their magnetic poſition. After this operation, he always found its efficacy, for a ſeaſon, conſiderably diminiſhed; for the efflu-

via

via of each bar, though increaſed in virtue, ſeemed not immediately to have acquired a communication with each other. However, it grew always more powerful after each of theſe operations; and it is more than probable, if a perſon could be found, who, with equal patience and ſkill, would, at proper diſtances, repeat the ſame proceſs, that the preſent machine would acquire a degree of force ſuperior to what the original ever poſſeſſed, for much depends upon time and a due poſition. If to theſe was added a freſh impregnation of each ſingle bar, by the means hitherto made uſe of, you would probably poſſeſs a larger fund of magnetic power, than exiſts in any artificial magnet now in being.

But if this cannot be obtained, if an able perſon cannot be prevailed upon to renew its vigour in this manner, it might poſſibly afford the curious ſome ſatisfaction, to know, whether, in its preſent ſtate, it loſes any force, or acquires freſh virtue; to know, with ſome degree of precision, how much weight it will now ſuſpend; and to obſerve annually its variation. I need not ſuggeſt, that a trial of this nature demands no ſmall attention. Even the motion of a carriage in the ſtreet, though at ſuch a diſtance as the ſociety's apartments, will make a conſiderable variation.

I do not know, that the doctor left behind him any deſcription of a compoſition he had made to form artificial load-ſtones. I have ſeen in his poſſeſſion, and many others of his friends have likewiſe ſeen, ſuch a compoſition; which

retained the magnetic virtue in a manner much more fixed, than either any real load-ſtone or any magnetic bar, however well tempered. In the natural ones he could change the poles in an inſtant, ſo likewiſe in the hardeſt bars; but in the compoſition the poles were immoveable. He had ſeveral ſmall pieces of this compoſition, which had ſtrong magnetic powers. The largeſt was about half an inch in breadth, very little longer than broad, and near a quarter of an inch thick. It was not armed, but the ends were powerfully magnetic; nor could the poles be altered, though it was placed between two of his largeſt bars, and they were very ſtrongly impregnated. The maſs, was not very heavy, and had much the appearance of a piece of black lead, though not quite ſo ſhining. I believe he never divulged the compoſition; but, I think, he once told me, the baſis of it was filings of iron, reduced by long-continued attrition with water to a perfectly impalpable ſtate, and then incorporated with ſome pliant matter, to give it due conſiſtence. Perhaps ſome of his acquaintance may have been more fully informed of this circumſtance; and it may be rendering great aid to future enquirers, to know every thing that can be collected relative to ſo curious a ſubject.

Leſt the machine itſelf ſhould again be expoſed to a like accident with that which deſtroyed a part of it, I thought an exact repreſentation of it, and its ſeveral parts, might be the beſt means of preſerving it to future times, if inſerted in the Tranſactions of the Royal Society.

E X-

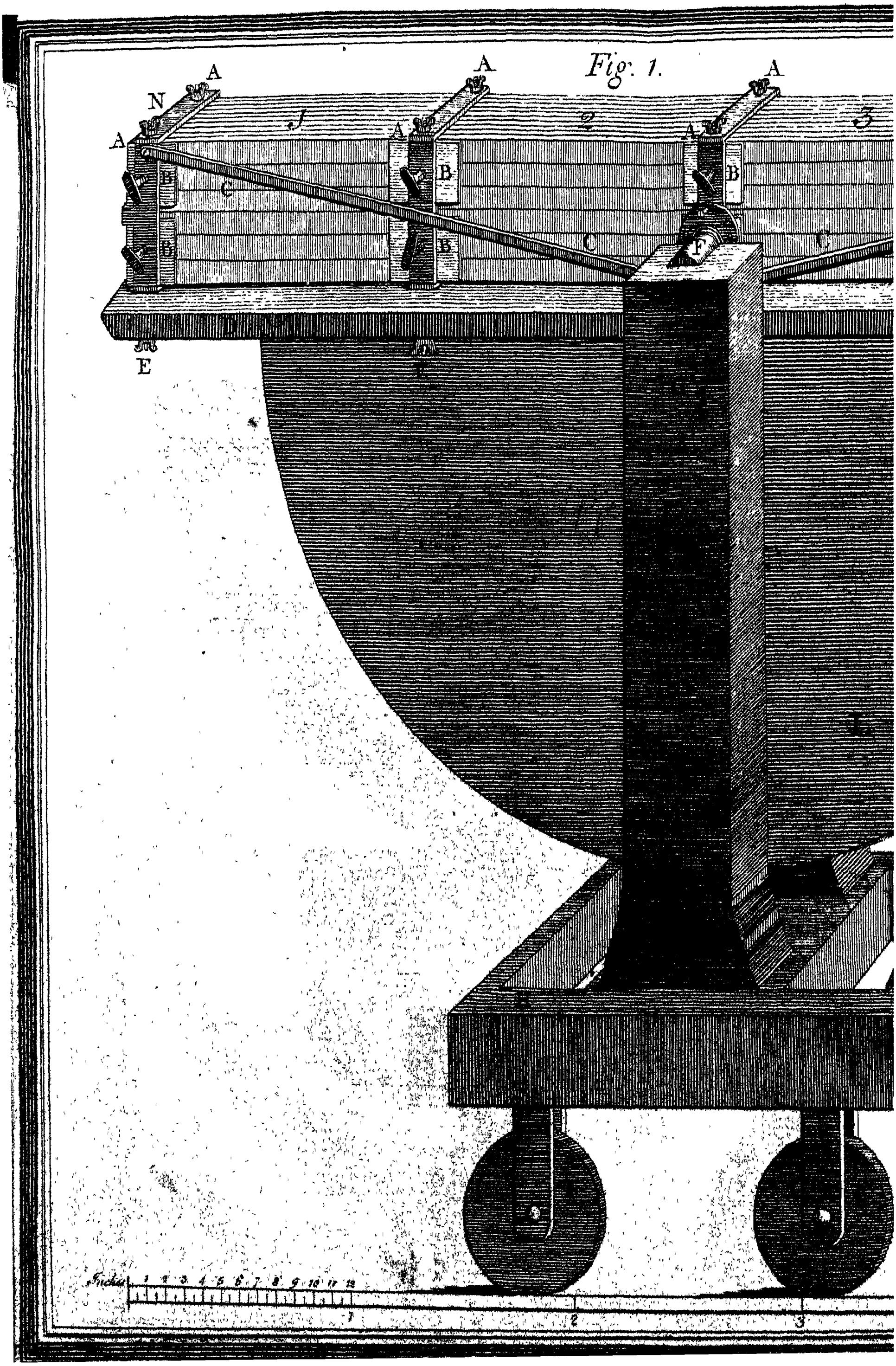
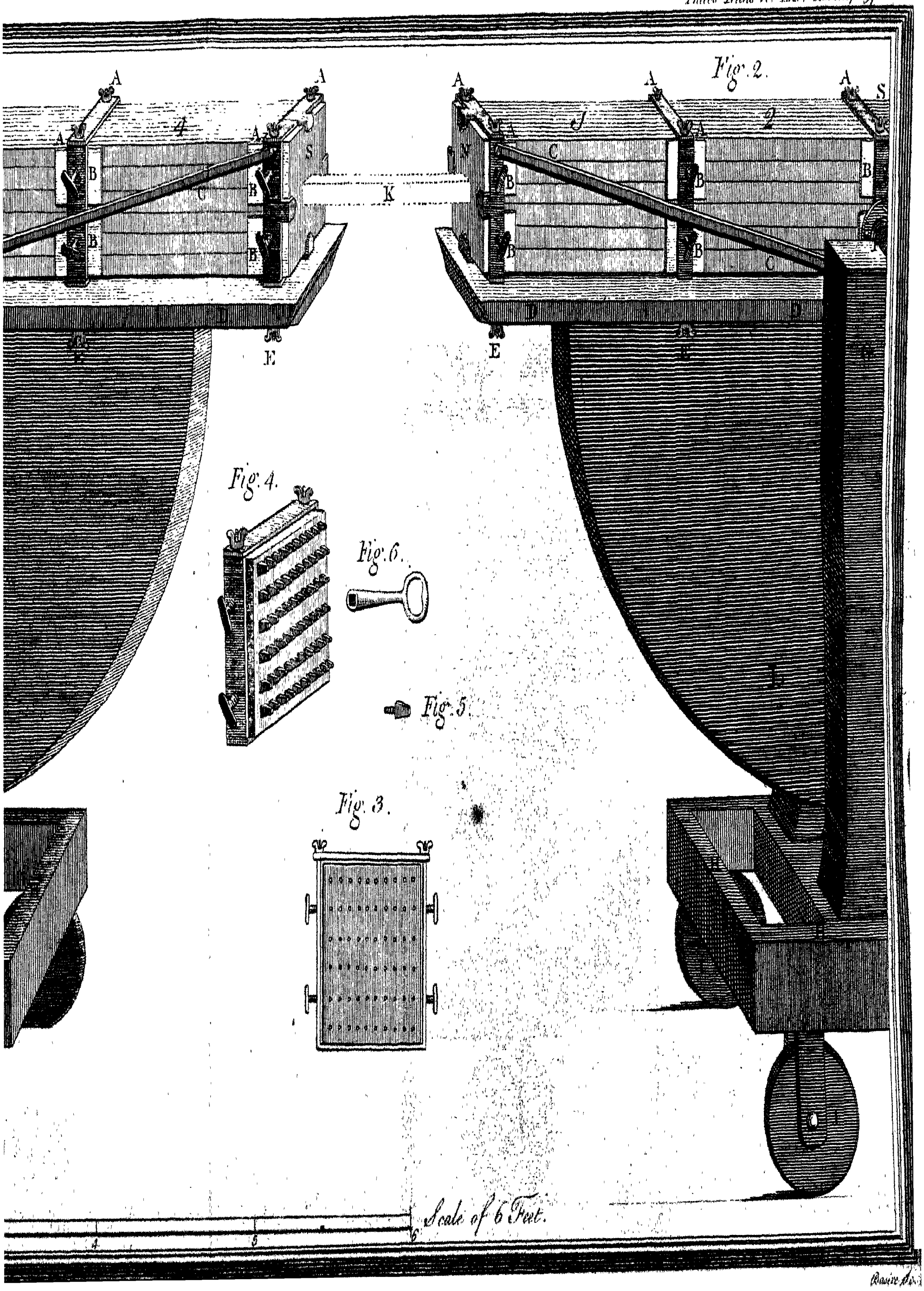


Fig. 1.
A
N
B
C
D
E
F
1
2
3
Inches
1 2 3 4 5 6 7 8 9 10 11 12
1
2
3



Basire Sc.

EXPLANATION OF THE PLATE.

Plate VII. ſhews the magazines according to the doctor's laſt diſpoſition of them. The two being perfectly alike, therefore fig. 2. contains only the half of one of them. Each magazine conſiſts of 240 bars, diſpoſed in four lengths, marked 1. 2. 3. 4.; every length containing ſixty bars, placed in ſix courſes or layers, in contact one on another; and ten in each courſe, placed ſide by ſide, in contact alſo. The bars being very nearly of a ſize, the ends of thoſe in one length are in contact with the correſponding ends of thoſe in the adjacent lengths. The magnetical North-ends of theſe bars, in each magazine, are all directed one way towards N; and the South-ends the contrary way toward S; thick plates of iron cover theſe ends N and S; the junction of the ends of the bars fall under the braſs braces AA.

As it has been found difficult, after the final hardening of theſe bars, to preſerve among them a perfect equality in ſize; therefore, the contact of their ſides are perfected by thin iron plates BB, ſlipped in between the braces AA and the junction of the ends of the bars: and theſe plates BB, being preſſed by the ſcrews paſſing through the ſides of the braces AA, keep the ends of the bars in as cloſe contact as their figures will permit; and, that the bars may be kept end to end in contact, the iron plate at the North-end in fig. 1. and at the South-end in

fig. 2. is perforated with fixty holes, one againft the end of each bar, as fhewn at fig. 3. with a fcrew fitted to each hole, as fhewn at fig. 4.: every fcrew having a fquare head as at fig. 5. may, by help of the key fig. 6. be turned, and, by preffing againft the end of the bar in the fourth length, force it againft its abutting bar in the third length, and fo on till the bars, end to end, are brought into contact and kept fo. The braces are in two pieces; the fides and bottom in one; and the other piece forms the top AA, which is held clofe to the bars by the fcrews paffing through it into the upright fides of the braces; and, to keep the braces at N and S fteadily in their places, the two long braces CC are affixed.

As each of thefe magazines weighed about 500 lbs. it became neceffary to have them fo placed as to be conveniently ufed. The doctor, therefore, by fcrews fixed the braces, containing the bars, to a ftrong mahogany plank DD, about $1\frac{3}{4}$ inches thick; the fcrews paffing through the plank entered the bottom parts of the braces AA. Againft the middle of the whole length, two ftrong brafs plates are well fixed to the fides of the plank; to thefe brafs plates are fixed two cylindrical gudgeons F, which projecting from the fides, like the trunnions of a cannon, lye in the fockets of the ftandard G, whereby the magazine eafily turns, as on an axis; and is fo well poized as to ftand in any inclination of the line NS; and in this the equilibrium is affifted by the ftrong mahogany femi-circular pieces LL, fixed in a vertical pofition to

the middle of the under part of the plank DD, on which the magnetic apparatus reſts. The ſtandards G are fixed to the ſquare frame HH, and the whole ſupported on the four trucks II, whereby the two magazines are eaſily brought end to end, or ſet at a convenient diſtance, ſo as to admit a bar K, to be placed between the ends, to be made magnetical.

XLIII. *A Demonstration of Two Theorems mentioned in Art.* XXV. *of the* Philosophical Transactions *for the Year* 1775. *In a Letter from* Charles Hutton, *Esq. F. R. S. to the Rev. Dr.* Horsley, *Sec. R. S.*

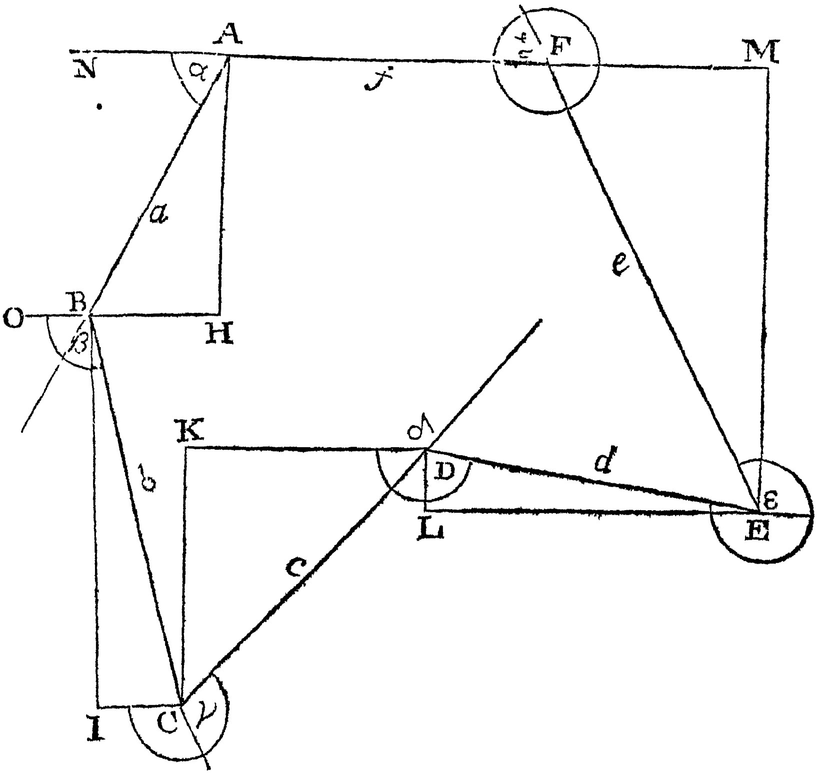
TO THE REV. DR. HORSLEY, SEC. R. S.

Woolwich Warren, Royal Mil. Acad.
April 17, 1776.

REV. SIR,

R. June 27, 1776. THE following is a demonstration of the two theorems mentioned in Art. XXV. of the Philosophical Transactions for the year 1776. If you think them proper to be inserted in the next volume of Transactions, be pleased to communicate them to the Royal Society for that purpose, from, &c.

THE



THE DEMONSTRATION.

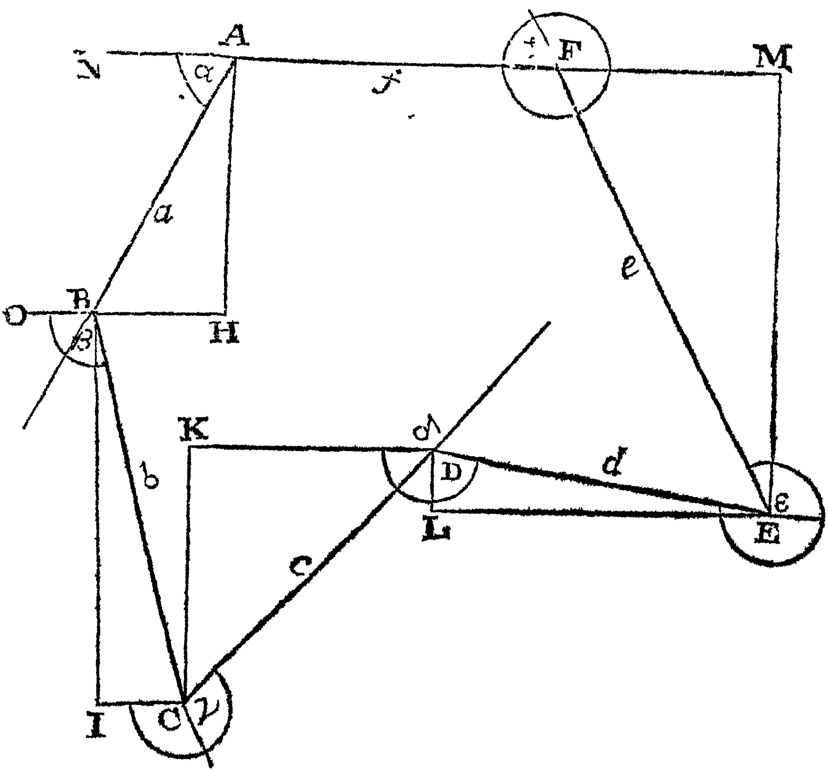
Parallel to one side, AF, of the polygon ABCDEF, through all the angular points, draw BH, CI, DK, EL, FM. Then are the several angles, which are made by these parallels and the adjacent sides of the figure, respectively equal to the sums of all the exterior angles of the figure, from the beginning to the place of each angle; by the beginning is meant either extremity of the side AF. Thus the $\angle$ NAB $= \alpha$, the $\angle$ OBC $= \alpha + \beta$, the $\angle$ ICD $= \alpha + \beta + \gamma$, the $\angle$ KDE $= \alpha + \beta + \gamma - \delta$ (for here δ, being without the figure, is subtracted instead of being

 added)

added), the $\angle$LEF $= \alpha + \beta - \gamma - \delta + \varepsilon$, &c. All this is evident from Eucl. I. 29.; the angles here spoken of being those, which are measured by the little arcs described about each angular point in the figure.

I. If right lines, AH, BI, CK, DL, EM, be drawn from the angular points, perpendicular each to the parallel which passes through the next angular point, the sums of the perpendiculars, drawn in contrary directions, the one upwards, and the other downwards, will be equal. And each perpendicular will be a fourth in proportion with the radius, that side of the polygon which is adjacent both to the perpendicular and the parallel on which it falls, and the sine of the sum of the external angles taken to that inclusively from which the perpendicular is drawn. Thus, AH + BI + DL = CK + EM, and rad. sin. α = AB : AH; and rad. sin. $\overline{\alpha + \beta}$ = BC : BI, and in like manner of the rest. Take the value, therefore, of each perpendicular by these analogies, putting unity for the radius, and subtract the sum of all that are drawn upwards from the sum of all that are drawn downwards, and the remainder, put equal to o, is the first equation; thus, AH $= a \times$ s. α (for $\angle$ ABH $= \angle \alpha$); BI $= b \times$ s. $\overline{\alpha + \beta}$ (for s. $\angle$ ICB = s. $\angle$ IBC = s. of its suppl. OBC or $\alpha + \beta$); in like manner, CK $= -c \times$ s. $\overline{\alpha + \beta + \gamma}$; DL $= d \times$ s. $\overline{\alpha + \beta + \gamma - \delta}$; EM $= -e \times$ s. $\overline{\alpha + \beta + \gamma - \delta + \varepsilon}$, &c. the last perpendicular will always be = o, because the sine of 360°, or of $\alpha + \beta + \gamma - \delta + \varepsilon + \zeta$ is nothing. Hence $a \times$ s. $\alpha + b \times$ s. $\overline{\alpha + \beta} + c \times$ s. $\overline{\alpha + \beta + \gamma} + d \times$ s. $\overline{\alpha + \beta + \gamma - \delta} + e \times$ s. $\overline{\alpha + \beta + \gamma - \delta + \varepsilon}$ = AH + BI − CK + DL − EM = o, which is the first equation.

II.



II. In like manner it appears, that the intercepted parts BH, CI, DK, EL, FM, AF, of the parallels before-mentioned, are equal to the several corresponding sides drawn into the cosines of the same sums of the exterior angles (the radius being 1.); and because BH−CI−DK−EL+FM+AF=0, therefore $a \times \text{cos.}\,\alpha + b \times \text{cos.}\,\overline{\alpha+\beta} + c \times \text{cos.}\,\overline{\alpha+\beta+\gamma} + d \times \text{cos.}\,\overline{\alpha+\beta+\gamma-\delta} + e \times \text{cos.}\,\overline{\alpha+\beta+\gamma-\delta+\varepsilon} + f \times \text{cos.}\,\overline{\alpha+\beta+\gamma-\delta+\varepsilon+\zeta} = 0$, which is the second theorem. Or, for the last term, $f \times \text{cos.}\,\overline{\alpha+\beta+\gamma-\delta+\varepsilon+\zeta}$, of this latter theorem, might be substituted its value f only.

EXPERIMENTS

MADE IN ORDER TO ASCERTAIN

The Nature of ſome Mineral Subſtances;

AND, IN PARTICULAR,

To ſee how far the Acids of Sea-Salt and of Vitriol contribute to mineralize Metallic and other Subſtances.

BY PETER WOULFE, F. R. S.

Who was nominated by the Preſident and Council to proſecute Diſcoveries in Natural Hiſtory, purſuant to the Will of the late HENRY BAKER, Eſq. F. R. S.

EXPERIMENTS, &c.

Gray's-Inn,
April 25, 1776.

R. June 20, 1776. THE late HENRY BAKER, Efq. a worthy member of this honourable Society, full of laudable zeal for the advancement of natural knowledge, has moft generoufly bequeathed the fum of one hundred pounds, for the ufe of this refpectable body; the intereft of which he intended fhould be annually applied for the benefit of fuch member whom the Council of this Society thought proper to nominate, for the tafk of making difcoveries in natural hiftory. Thefe gentlemen have this year done me that honour, which I moft gratefully acknowledge; and in confequence of this appointment, I have made experiments on a variety of mineral fubftances.

IN order to ascertain how far the method, I proposed to pursue in the following experiments, was calculated for the purpose of making a proper analysis of the minerals I here treat of; and particularly to discover the existence of the acids of salt and vitriol in them; I thought it expedient to make some artificial preparations, which I judged to be similar to the natural substances, and I submitted them to the same trials, and in similar quantities; for as many of the minerals, which are the subject of the present paper, are exceeding scarce, I was under the necessity of using in these experiments smaller quantities of them, than I should otherwise have em-

[*a*] With that intent, I dissolved half an ounce of refined silver in pure *aqua fortis*, and made a *luna cornea*, by adding to it a solution of sea-salt in water; this was well edulcorated, dried first in the air, and afterwards with a strong heat, but not so great as to melt it; the *luna cornea*, thus obtained, weigh five drams and one scruple, which is one-third more than the original weight. I must here observe, that I have used Troy-weights in all these experiments.

[*b*] The like quantity of the same silver, dissolved in the same acid, and precipitated with a solution of tartar of vitriol, and treated as at [*a*], weighed five drams and twenty-two grains. It is worthy of observation, that if *sal polychrest* be made use of in the place of tartar of vitriol, its precipitate will not exceed three drams and fifty-four

four grains; but if acid of salt, or a solution of sea-salt, be added to the washings of this precipitate, the remainder of the silver is preciptated, and forms a *luna cornea.* This shews, that there is a difference between tartar of vitriol and *sal polychrest*, notwithstanding what chemists think to the contrary; and indeed, upon trial, I found the *sal polychrest* to contain a small portion of liver of sulphur. Silver dipped into a warm solution of this salt becomes instantly black; thus the *sal polychrest* may be readily distinguished from tartar of vitriol, which does not tarnish silver.

The precipitate of silver, by tartar of vitriol put on a red-hot iron, melts and grows liquid like *luna cornea.* The precipitate of silver by *sal polychrest*, treated in the same manner, does not grow so liquid, but boils up and at last dries.

[*c*] Half an ounce of lead, dissolved in *aqua fortis*, and precipitated by a solution of tartar of vitriol, edulcorated and dried, weighed five drams and a half.

[*d*] The like quantity of lead, dissolved as at [*c*], precipitated with a solution of sea-salt, edulcorated and dried, weighed only half an ounce and eighteen grains. A solution of tartar of vitriol, added to the washings of this precipitate, caused a fresh precipitation, which, after edulcoration and exsiccation, weighed forty-two grains. Hence we see, that lead united to acid of salt is soluble in water, and on that account so small a quantity of precipitate was obtained; but the tartar of vitriol precipitating its washings (for the acid of vitriol does the same) shews, that

that lead has a greater affinity with this acid than with that of sea-salt; it also shews, that this combination of lead with acid of vitriol is insoluble. That most excellent chemist Mr. MARGRAF, in his experiments on the Bolognian *phosphorus*, has shewn how the Bolognian stone and other such spars, as well as the *gypsa*, are decomposed by fixed alkalies; but, as it was necessary to employ an excess of alkaly to decompose them thoroughly, the quantity of neutral salt he thereby obtained could not be accurately ascertained, on account of its mixture with the alkaly. I so far availed myself of this learned chemist's method; but contrived to remove that inconveniency, in such a manner that the excess of alkaly was separated, and the neutral salt left pure. This consists in saturating the salt with distilled vinegar, evaporating the mixture slowly to dryness, and dissolving the *sal diureticus*, formed by the combination of the vinegar and excess of alkaly, in rectified spirit of wine, for this salt is very soluble in it; whereas the combinations of the acids of vitriol and of sea-salt, with either vegetable or marine alkaly, are no way soluble in this spirit. I must here observe, that no fixed alkaly is quite free from neutral salt; but the purest is that made with good tartar. Two drams of the alkaly, I made use of for my experiments, contained two grains of tartar of vitriol, for which I made an allowance in all my experiments. The quantity of neutral salt in any alkaly may be accurately ascertained by this method; and I can take upon me to say, that

that a single grain of neutral salt in an ounce of *alkaly* may be discovered by this process.

EXPERIMENT I.

Two drams of the horn silver [*a*] were well ground in a glass mortar, with an equal quantity of fixed alkaly of tartar, and distilled water enough to make the mixture of a soft consistence; it was then taken out of the mortar, and deprived of its moisture in a china cup fixed in a sand heat. The mixture was then powdered and put into a common green ounce phial, and this into a crucible, and surrounded with sand up to its neck. The crucible was fixed in a proper furnace, and a fire made round it, which was increased by degrees until the phial became of a dull red colour, in which state it was kept for an hour. The crucible was then taken out of the fire, and, when quite cold, the phial was broken; care was taken to separate the matter from the broken bits of glass. This matter was of a spungy texture, and would not powder, but flatted in the mortar, and then shewed its silvery appearance; it was, therefore, cut into thin slices and digested with four ounces of distilled water; the solution was poured off and filtered, and four ounces more of the like water added to what remained undissolved; this also, after digestion, was poured off, filtered, and added to the first solution. By this means the silver was deprived of all its saline part. The solution was evaporated slowly to about one-half, and then saturated with distilled vinegar; this was afterwards filtered, gently evaporated

evaporated to dryness, and freed from its *sal diuretticus* by three lotions with rectified spirit of wine. The neutral salt here remaining was dissolved in a small portion of distilled water, then filtered and put into a wine-glass, covered with filtering paper, to keep out the dust. In about three months it was quite dry, and consisted of small, flat, cubical crystals of regenerated sea-salt, which altogether weighed fifty-five grains.

EXPERIMENT II.

Two drams of the precipitate of silver, by tartar of vitriol [*b*], treated as in the first experiment, produced one dram and seven grains of brown tartar of vitriol.

EXPERIMENT III.

Two drams of precipitate of lead, by tartar of vitriol [*c*], treated as in the first experiment, produced one dram and five grains of brown tartar of vitriol.

EXPERIMENT IV.

Two drams of precipitate of lead, by sea-salt [*d*], afforded one dram and one grain of cubical crystals of regenerated sea-salt.

EXPERIMENT V.

Two drams of *mercurius dulcis*, treated in the like manner, produced thirty-eight grains of cubical crystals of regenerated sea-salt.

EXPE-

EXPERIMENT VI.

Two drams of turbith mineral (made by precipitating a solution of quicksilver in acid of nitre by a solution of tartar of vitriol, well edulcorated and dried), treated as in the first experiment, produced thirty-six grains of brown tartar of vitriol.

In the first and second experiments, the silver regained its metallic form; in the third and fourth the lead was reduced to the state of massicot; and, in the fifth and sixth the quicksilver was totally dissipated.

From the foregoing experiments it is clear, that combinations of the acids of sea-salt and of vitriol with silver, lead, and quicksilver, are decomposed by these means; and also, that the quantity of neutral salt, which their acids form by combination with the alkaly, is ascertained. Having premised these experiments on artificial substances, I now come to those made with natural ones, which are the subject of the present paper.

OF NATIVE HORN SILVER.

Horn silver is found of various colours, *viz.* green, yellow, brown, purple, and also black. When crystallized, it forms perfect cubes; it readily melts when put upon a red-hot iron, but does not smoke; it may be easily cut with a knife, for it is somewhat malleable; we must, however, except the black sort, which is brittle, and may be powdered. Some authors will have it, that this

this mineral is composed of silver, sulphur, and arsenic; some, of silver sulphur, arsenic, and fixed alkaly; others, of silver, arsenic and acid of sea-salt; but Mess. CRONSTEAD and LE SAGE assert, that it is composed of silver and acid of salt only. I know of no experiments that have been made public with a view to determine this affair, except those of M. LE SAGE; but he and I differ widely in the result of our experiments.

EXPERIMENT VII.

Two drams of brown, native horn silver, cut into very thin slices, and well ground with the like quantity of fixed alkaly of tartar and a little water, treated as in the first experiment, produced forty-three grains of neutral salt, composed of flat cubic crystals of regenerated sea-salt, intermixed with brown crystals of tartar of vitriol; this last did not appear to be above one-third of the first.

The brown colour of the horn silver is owing to an ochre of iron; a thin slice of it, viewed through a magnifying glass, looks in some parts of a smooth pearly colour, but in others of a powdry brown one.

The silver, which remained after this operation, had the same texture and silvery appearance as that of the first experiment. It was digested with distilled vinegar, then filtered and evaporated to one-fourth, with a design to dissipate the excess of acids. A fresh infusion of galls mixed with it, becomes of a dark purple colour, which precipitates; an incontestible proof of the iron it contains;

Silver, in its metallic ſtate, is not ſoluble in diſtilled vinegar.

EXPERIMENT VIII.

Two drams of the pearl-coloured horn ſilver, treated with the like quantity of fixed alkaly as in the firſt experiment, produced fifty-one grains of neutral ſalt, compoſed of flat cubic cryſtals of regenerated ſea-ſalt, with a mixture of brown cryſtals of tartar of vitriol; this laſt, in appearance, is about a fourth or more of the firſt. The ſilver here remaining had the ſame appearance as that of the firſt experiment; it alſo gave marks of its containing iron, but not near ſo great a quantity as that of the ſeventh experiment.

EXPERIMENT IX.

The brittleneſs and colour of the black horn ſilver made me at firſt doubt of its being horn ſilver; and I thought, that, if it contained any, it muſt be mixed with ſome other mineral ſubſtance. Volatile alkalies having the property of diſſolving all combinations of ſilver with acids, I availed myſelf of that well-known property on this occaſion. I took, therefore, ſeven drams of the black horn ſilver, and digeſted it at three different times with a large portion of volatile ſpirit of hart's-horn; this ſpirit I preferred to that of *ſal ammoniac*, as I knew it to be free from acid of ſalt. The three ſolutions were mixed, filtered, and ſlowly evaporated to dryneſs, and produced two drams and two ſcruples of a dark ſlate-coloured horn

ſilver.

ſilver. Forty-ſix grains of artificial horn ſilver [*a*], diſſolved in ſpirit of hart's-horn, and dried in the ſame ſlow manner, increaſed only two grains in weight, and was of the ſame dark colour; owing, no doubt, to a ſmall portion of volatile ſpirit adhering to it.

The two drams and two ſcruples of horn ſilver, obtained by means of the ſpirit of hart's-horn, were well mixed with an equal quantity of ſalt of tartar; and being treated as in the firſt experiment, produced one dram and eleven grains of neutral ſalt, conſiſting of flat cubical cryſtals of regenerated ſea-ſalt, intermixed with brown cryſtals of tartar of vitriol, which laſt appeared to be in leſs proportion than in the former experiments. The ſilver, after this operation, had the ſame ſpongy, ſilvery appearance as in the former experiments. The undiſſolved part of of the horn ſilver, which remained after its horn ſilver was extracted by the ſpirit of hart's-horn, retained its black colour. It was calcined in a crucible, and, during the calcination, a ſulphureous ſmell was obſerved; the calcination was continued until this ſmell ceaſed. It had now an aſh-colour, ſilvery appearance, and, during the operation, ſlightly caked together, which made me think it had yet ſome horn ſilver; I therefore digeſted it with more ſpirit of harts-horn; and, having evaporated the ſolution ſlowly to dryneſs, I obtained thirty-four grains of horn ſilver; the undiſſolved part here remaining, being melted with black flux, produced two drams and a half of pure ſilver, ſo that black horn ſilver is compoſed of horn ſilver and ſulphurated ſilver. The ſolution of the

the three ſorts of horn ſilver in ſpirit [illegible] were as colourleſs as water; a proof they contain no copper.

To give a proof of the exiſtence of the acid of ſalt in the horn ſilver, I took four grains of the cubical cryſtals obtained in the ſeventh, eighth, and ninth experiments, and having put them into ſeparate wine-glaſſes, poured a little oil of vitriol on each, which made them all boil up, effervesce, and ſend forth copious fumes of acid of ſalt, juſt as the like quantity of ſea-ſalt would have done.

In order to give alſo a convincing proof, that the horn ſilver contained acid of vitriol, I availed myſelf of M. MARGRAF's diſcovery, who ſays, that a ſolution of calcareous earth in acid of nitre is precipitated by a ſolution of tartar of vitriol; for the acid of vitriol forſakes its alkaly to unite, and form a ſelenite with the calcareous earth. I therefore diſſolved twelve grains of tartar of vitriol in diſtilled water, and having filtered the ſolution, I added to it a ſufficient quantity of a ſolution of chalk in acid of nitre, which cauſed a precipitation; this precipitate, being edulcorated and dried, weighed ſeven grains: this ſerved as a comparative experiment. It is probable, from the ſmall quantity of this precipitate, that the whole of the tartar of vitriol was not decompoſed. It ſhews, however, that ſeven grains of this precipitate require twelve grains of tartar of vitriol to their formation.

The neutral ſalt of the ſeventh experiment, treated in the like manner, produced eight grains of ſelenite; that

of

of the eighth, ſeven grains and a half; and that of the ninth ten grains.

In calcining the different ſpecimens of horn ſilver with the alkalies in the foregoing experiments, I could perceive no ſmoke or ſmell iſſue out of the phials. Hence I conclude that the horn ſilver contained no arſenic; and that none of it was diſſipated during the operation.

From the foregoing experiments it appears, that the horn ſilver is compoſed of ſilver united to the acids of ſalt and of vitriol; and that this laſt is nearly one-third of theſe firſt.

OF HORN MERCURY.

I diſcovered this mineral about four years ago, when I was collecting minerals at Obermoſchel, in the Dutchy of Deux Ponts. I have ſince ſeen it in a fine collection abroad; but no one ſuſpected what it was until I had made it known, for it was taken for an inſignificant ſpar. I have found this mineral of three colours; white, and of a ſhining brightneſs; yellow, and alſo black; this laſt owes its colour to a mixture of minute particles of live quickſilver. This ſubſtance cryſtallizes in various forms; but the cryſtallization is too ſmall to be deſcribed without the help of a microſcope.

EXPERIMENT X.

I took three drams of horn mercury, picked as clean as poſſible from the cinnabar and ſtony matter to which it adhered, and treated it with two drams of ſalt of tartar

as in the first experiment. The quantity of neutral salt here obtained was only half a dram, and it was composed of flat, cubical crystals of regenerated sea-salt, intermixed with about an equal quantity of brown crystals of tartar of vitriol. The quicksilver was all dissipated in the operation, as in the fifth experiment. Four grains of the cubic crystals, mixed with oil of vitriol, boiled up, and sent out copious fumes of acid of salt. The remainder of the neutral salt was dissolved in distilled water, and mixed with a sufficient quantity of a solution of chalk in acid of nitre; the precipitate, here obtained, being edulcorated and dried, weighed eight grains and a-half. Hence we may conclude, that this mineral is composed of quicksilver united to a greater proportion of acid of vitriol, than of acid of salt.

The horn-mercury used for this experiment was intermixed with minute *globules* of quicksilver, which could not well be separated, to which the small produce of half a dram of neutral salt was owing; and, indeed, to obtain the three drams of horn-mercury employed for this experiment, it was necessary to destroy several beautiful specimens.

The ingenious M. LE SAGE, of the French Academy of Sciences, has published many experiments to shew, that the acid of salt contributes to mineralize a great variety of mineral substances; but I have tried a great number of them without obtaining an atom, either of acid of salt, or acid of vitriol. Among these are the

 following,

following, which were ſubmitted to the ſame trials as in the firſt experiment.

White ſpathoſe iron ore from Bayreuth.

Corniſh and Bohemian tin grains.

Mendip calamine.

Semi-pellucid calamine, from Wales.

Sooty kobalt, from Saxony.

Somerſetſhire mangancſe; this is the only ſort I ever ſaw which is mixed with calcareous ſpar, and, on that account, effervеſces with acids without help of heat.

Ceruſſa nativa, in lump, from Lorraine; I found this to conſiſt of a *calx* of lead, mixed with an argillaceous earth.

White, tranſparent, lump lead ore, from Somerſetſhire.

Cat's-tooth white lead ore, from Ireland.

White lead ore, from Poulaſent in Low Brittany.

Black, whitiſh lead ore, from Tſchoppan in Saxony.

Green lead ore, from ditto.

Green lead ore, from Freibourg in Briſcau.

In all theſe experiments the lead was converted to maſſicot; but, in the two laſt, the maſſicot had a pale greeniſh caſt, owing to iron, and not to copper: for if theſe green lead ores, after having been calcined with fixed alkaly and edulcorated, be digeſted with acid of vitriol, we obtain a greeniſh ſolution; this being then deprived of its exceſs of acid by an alkaly, filtered and mixed with infuſion of galls, gives a black, inky colour.

Having now tried in my manner, without ſucceſs, a great number of the minerals which M. LE SAGE affirms to contain the acid of ſalt, it became alſo neceſſary to try his method; and I choſe ſuch ſubſtances as he ſays are the moſt replete with that article.

EXPERIMENT XI.

White, ſpathoſe, iron ore from Bayreuth, tried in M. LE SAGE's manner.

I put into a ſmall glaſs retort three ounces of this mineral powder, and poured on it an equal quantity of oil of vitriol. The retort was placed in a proper reverberatory furnace, and a quilled receiver luted to it; an ounce meaſure of oil of tartar, *per deliquium*, was previouſly put into the receiver, and ſhaken ſo as to moiſten all its internal parts: a very ſlow fire was made under the retort, ſuch as cauſed no moiſture to riſe; and this gentle degree of heat was continued for five hours. In a little time the upper part of the receiver was lined with long ſpiculine cryſtals; and, after the operation, the oil of tartar, which was at the bottom of the receiver, was, for the moſt part, cryſtallized.

EXPERIMENT XII.

Somerſetſhire mangancſe, treated in the like manner, produced the ſame ſpiculine cryſtals in the upper part of the receiver; and the oil of tartar was, in like manner, cryſtallized. It was here neceſſary, on account of the

 effervefcence

effervescence of the mixture, to make use of a larger retort, and to let it stand for twelve hours before any fire was made under it, in which time the spiculine crystals appeared.

EXPERIMENT XIII.

Somersetshire white-lead ore, treated in like manner, shewed the like appearances; but here there was only a very slight effervescence.

EXPERIMENT XIV.

Cornish tin-grains, treated in this manner, caused no change in the oil of tartar; nor was there the least appearance of crystallization in the receiver.

EXPERIMENT XV.

White spathose iron ore, distilled *per se* with a strong fire, shewed the same appearance of crystallizations in the oil of tartar and in the upper part of the receiver, as when distilled with oil of vitriol; see the eleventh experiment.

I now saturated each of the crystallized alkalies of the eleventh, twelfth, thirteenth, and fifteenth experiments, with distilled vinegar, and having filtered them, they were slowly evaporated to dryness, and dissolved with rectified spirit of wine, as in the first experiment, but no neutral salt was left; from hence we may certainly conclude, that these minerals contained neither acid of salt, nor acid of vitriol. The cause of the crystallization of the

the oil of tartar is owing to the phlogiſtic fixed air of the the minerals; but of this I ſhall treat more fully in another paper; and will now only add, that chalk, diſtilled *per ſe*, with a ſtrong fire, makes the oil of tartar in the receiver alſo cryſtallize. I muſt, however, own, that no cryſtallization is obſerved in the upper part of the receiver; nor was it to be expected, on conſidering the moiſture which the chalk affords in diſtillation.

M. LE SAGE ſays, that the cryſtallizations in the upper part of the receivers in his experiments were compoſed of cubic cryſtals; but in all mine they were ſpiculine.

By the foregoing experiments it appears, that ſilver and quickſilver are the only ſubſtances which are mineralized by the acid of ſalt, and that they are alſo combined with acid of vitriol.

Though the reſult of my experiments has been very different from that of the ſame ſubſtances examined by M. LE SAGE, yet I have too high an opinion of him, and know him too well, to call his veracity in queſtion. I am rather inclined to ſuſpect that this difference may proceed, from his having uſed oil of vitriol that contained acid of ſalt. Mr. HOLKER, who prepares this acid in France, owned to me, that he had, in his firſt tryals, mixed ſea ſalt in the preparing of it, with a view to increaſe its quantity. Might not M. LE SAGE have made his experiments with ſuch an acid; and of conſequence obtained acid of ſalt from the ſubſtances he tried.

PRE-

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A.

Cigma

Days,

D.

E.

Elans

F.

Franklin

G.

H.

I.

Italy,

Lions,

M.

O.

P.

Phoſphorus

S.

T.

A com-

will

U.

V.

W.

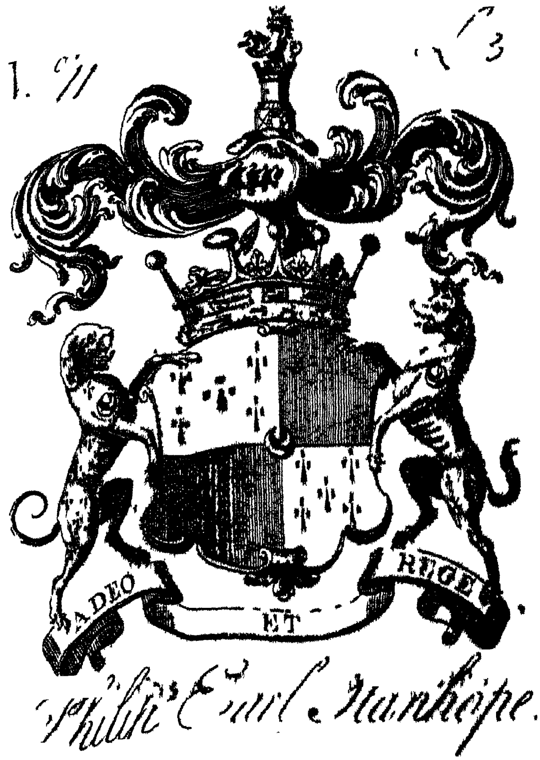
Wax,

The End of the SIXTY-SIXTH VOLUME.

The Number of PLATES *in this Volume is* SEVEN

E R R A T A.

Page	Line
15.	1. *n.* *for* branche *read* branches.
120.	12. *for* part this *read* part of this.
231.	14. *for* expement *read* experiment.
32.	14. *for* ſuphureous *read* ſulphureous.
242.	3. from the bottom, *for* oommon *read* common.
	2. from the bottom, *for* cne *read* one.
262.	laſt word of the page, *for* Both *read* Theſe.
416.	laſt word of the page, *for* Nor *read* Now.
445.	12. *for* unhealty *read* unhealthy.
498.	in the note, *for* they do not admit a dark hair *read* they do not by choice admit a dark hair.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